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DEMONSTRATION OF OPHTHALMOLOGICAL AND OTHER INSTRUMENTS.

9th June, 1910.

MR. REINER, who was called on to open the proceedings, said that he thought it advisable to put on the blackboard an outline of what he proposed to speak about, for the purposes of the discussion.

With regard to the first subject, "The Method of Determining the Optical Centre of a Lens," he had brought an instrument for the purpose of demonstration. This instrument had been kindly lent to him by Mr. Dixey, and it was most useful for centering lenses. Probably Mr. Dixey would explain a little more about it than he (the speaker) could. A cross line used with a pair of spectacles, having a small aperture for one eye, to assist accommodation, was also a very good method.

With regard to the second point, "The Importance of Correctly Determining the Visual Centres," Messrs. Carl Zeiss had very kindly lent him their interpupillary gauge, which could be seen by the members presently. That was used by placing it against the patient's eyes, and from the scale the exact interpupillary distance could be read. Messrs. Zeiss also gave a scale by which could be calculated both the distance and reading centres. That had been further elaborated in another chart which he would show them. This chart gave the centres for reading in one column, and in another column the centres for distance, which were very often wanted. They also gave the ratio, so that the operator could work out the distance for himself. Usually he thought there was a rough

method of allowing about two mm.; the chart showed only one measurement which agreed with this allowance, viz., 54 and 58. In the matter of obtaining the exact visual centres in fitting lenses, as a rule this was rather difficult, because he usually found (and he believed this was the experience of opticians generally who had to deal with prescriptions) that there was no basis to start with. He thought opticians should consider that point, as there were very few who took care to have an instrument of precision for measuring the visual centres. The measurements, as usually taken with the rule, were very crude, and he was quite sure many mistakes were made. They would probably remember that Mr. Sutcliffe showed an instrument at a meeting of the Society last year for determining the visual centres. This was an excellent instrument, and it also took account of another factor, the angle gamma, and gave some indication as to which was the real and which the apparent visual centre. He believed Dr. Maddox had a new instrument, and in order to describe it he had got Messrs. Carl Zeiss to lend him their exophthalmometer, which he believed was something similar in form. It was a method of using a mirror, and he believed it was almost the same as Dr. Maddox's instrument, except that this was a gauge to take the measurements of the profile of the cornea at intervals of time.

With regard to another point, "The Reading Centres in 'Constant Wear' Lenses." To take his own case, he was wearing a plain lens in the right eye and a cylindrical lens of about one diopter in the left. This was prescribed for constant wear. He certainly had the distant centres correct, but on looking down he was entirely out of the centres. What should be the remedy? If there should be correct centering why should he not be provided with lenses which would give him exact centres for reading? Having a cylindrical lens of about one diopter, he

certainly got a small prismatic effect at 5 mm. below the centre. In the other eye the plain lens had no effect, so that he was under a certain amount of strain. If those who contended that there should be distinct reading centres were right, it should be considerable. He should like to have that point fully discussed. To take the case of one of the members who was wearing a minus lens in one eye and a plus lens in the other, and one power for all purposes. What took place when he looked down for reading? He would be in a worse plight than was the speaker. Perhaps some remedy might be suggested in the discussion.

The same question applied to bi-focal lenses. A good deal was said about bi-focal lenses being incorrectly centered, and the admission was made that in many lenses no centres could be found. Why should there not be a common centre, or why should not the centres be arranged similar to those in the lenses which were prescribed for constant wear? He only had one centre, therefore why should not bi-focal lenses have one centre also? In bi-focal lenses, if with a common centre they would lose that movement which was present when passing from the distance to the reading glass. He did not think he had heard anyone discuss the question of movement, and it was really a very important one.

The last question might seem a little strange, "Should lenses have a plurality of centres?" He put that forward as a suggestion. Would it be possible to make a lens which would have a multitude of very tiny lenses that would give a different centre at any point, and so arranged that at any point the person was looking through he would get a distinct small lens? He would like to know what the effect would be. Probably some suggestion might be made in the course of the discussion.

MR. DIXEY said that he had listened with much interest to Mr. Reiner's remarks. His

difficulty was not so much what to say, as what not to say in the discussion. He would follow on Mr. Reiner's lines, and would first of all say something about the little instrument which was borrowed from himself. The principle involved in that little instrument was exactly the same as in all centering instruments—exactly the same as in the more elaborate ones. It was not what would be used in practical work; as a matter of fact, the instrument used in his workshop was made out of a small kitchen table with a hole cut in the top and an adjustable arrangement underneath. That particular instrument was a little portable thing, one of about three dozen which he made some years ago in response to a special request. The idea was that they would be useful for testing work done either by country opticians or oculists, and a good many of them were distributed over the country for that purpose. It was quite a practical instrument, and a great number of people used it. With regard to the American instrument, it was a very beautiful one, and was much more elaborate, but one objection he should have to using it was that it was all arranged vertically instead of horizontally. He thought it was much more convenient for work generally to have the lens which was being examined on a horizontal surface. It could be adjusted better, and another thing he regretted in that particular instrument was that there was no mechanical arrangement for rotating the lens, and it had to be done with the finger.

Following on Mr. Reiner's lines, with regard to the importance of correctly determining the visual centres. Very good results were got with an ordinary rule if one knew what one was doing. The expert optician measuring visual centres ought to be able to recognize a wide angle alpha. He ought to have some regular method of estimating exactly what amount of convergence his customer is exerting, and he should know exactly what point he is taking his

lines from. He may work either with two eyes or with one, but he must be very careful when taking two lines from one point to allow for parallax, and not to permit any shifting of either pair of eyes involved in the measurement. That, of course, was all very obvious, but it was something which wanted care and practice. If they got bad results, as a rule, it meant that the person using the rule was not sufficiently practised with it, and had not had the elementary method involved in the measurement properly explained to him. With regard to the other devices, the objection to them was that they were subjective instead of being objective. With an intelligent person one could rely on subjective measurement, but as these conditions did not always exist very queer results would be obtained in using an instrument of that kind. In using any reflecting instrument such as that of Messrs. Zeiss, no allowance was made for the angle α, and, of course, they should always recollect that when they had got the parallel position for visual lines, an adjustment of the lens must be made for any convergence that might have to be exercised. With regard to the difference in visual centres, the different visual distances, he did not think it was generally recognised what an enormous difference there was in the distance apart of the visual lines at the plane of the spectacles on the face and in their parallel position. Mr. Reiner did not exaggerate at all when he said that in his case the difference was 5 or 6 mm. This fact did not surprise him (the speaker), although it would a great many people. Especially in convex reading lenses that was a very important point, because if anything, convex reading lenses ought to be centered inside the visual line instead of outside. Mr. Reiner had raised an interesting point about "constant-wear" lenses. Of course, all those things were very elementary, but they involved a lot of technical matter which most of them had at their fingers' ends, and one did not

know what to explain and what not to explain. One very simple consideration which had a bearing upon that point was the difference between convex and concave lenses. Say they had two or three diopters of equal degree of convexity in each eye, both lenses similar in the two eyes, and suppose they had a case of hypermetropia, in which the correction was plus 3 diopter in both eyes. There they had two measurements to consider. The distance measurement might be, say, 64, and the reading measurement 59 mm. for the distance, apart from the visual line in the plane of the spectacles. What he wanted to point out was that the proper centering of the lenses would be 59 mm., or even less—58 he would make them, to be on the safe side—because the thing to avoid was getting the convex lenses too wide and conversely concave lenses too narrow; and for the same reason the thing to avoid in both cases was undue convergence of the eyes. With concave lenses it worked the other way. Supposing they had a simple case of concave correction. There they made their lenses to the larger width of pupil to the 64 mm., because then when the eyes were converged and the visual lines were 59 mm. apart, then that would strike the lenses in such a way that they would get convergent prisms, and they would get their convergent system—they would be able to read with less convergence effort instead of more, which was the thing to avoid. What then should be the relative position of the reading centres and distance centres in bifocals? He could not quite see Mr. Reiner's point about the plurality of centres. They would always have a prism edge which would give a jump effect, which would be the great difficulty in having a multiplicity of lenses in one spectacle area; there would also be the difference in the convergence and different width in visual lines in treating different distances; but it was quite easy to have double lenses, and it was even workable and possible to

have triple lenses. He supposed everybody had seen triple lens spectacles. With regard to the relative position of reading distance centres, he did not think there was anything very complicated about that. The simple rule was the same as that which one used in fitting spectacles. They must allow for the difference in the distance apart of the visual lines when the eyes were converged and when they were looking at a distant point. If they erred they must err in the direction which he had already mentioned, *i.e.*, in getting the convex lenses too narrow and the concave lenses too wide. They had to use their intelligence very considerably in some of those cases. When they got a case such as Mr. Reiner had instanced, where the left eye was minus 1 and the right eye was plane, they could put the centre where they liked, because the centre of the plane lens was all over the lens. It was not right to say that it had no centre, as the centre was everywhere. Of course, there was some prismatic effect in oblique sight. But there was no real prismatic effect in the plane lens which would cause any strain. But if instead of a plane lens they had a minus 3 spherical lens in one and a plus 3 in the other; then it was a very serious consideration how to arrange it. What they had to consider was whether it was better to unduly converge or unduly diverge the eyes in each case. And as a matter of fact, with spectacles such as those, if they were correctly centered from the measurements taken, no such good result was obtained, as if they were centered with an intelligent view of the different lenses and of the separate effects of the different parts of the lenses which must be used. Another difficulty was with cylindrical lenses of oblique axes. There one got trouble sometimes when the lenses sloped a particular way, though sometimes the obliquity assisted the arrangement of the lenses because they got narrow centres for the lens in the lower part and wider in the upper part; but sometimes it

was the other way, and then their difficulties began. The best rule was to remember, as he said before, to avoid undue convergence of the eyes. He thought there were two things that every spectacle fitter ought always to have in mind. One was that spectacles were always a compromise. Even if they made to the simplest formula they had to sacrifice something—sometimes it was a very serious sacrifice—and what they had to aim at in the first instance was to get the correct measurement and adjustment, but if they wanted to do fine spectacle fitting they must determine the most suitable compromise, so as to get the best effect from the spectacles for the particular case and the particular conditions of the pair of lenses they were putting up. And the second thing to remember was *de minimis non curare*, as they say in the law, not to worry about very small departures from ideal perfection, as they could not get it, but they should aim at getting as near to it as they could.

Dr. Clay said he had been trying to puzzle out how the apparatus referred to by Mr. Reiner worked. It seemed to him—he did not know whether he was right—that it drew a virtual image on that side (describing) so as to get rid of the parallax.

Mr. Dixey said Dr. Maddox's idea was a very good one, and could be arranged quite easily. (Mr. Dixey then described the idea.)

Mr. Mackinney said that the question which interested him was No. 5. He had had some experience in the manufacture of fused bifocal lenses, and had often been in doubt as to where he should put the optical centre in certain combinations of distance and reading lenses. If they took that (describing on the blackboard) as the position of the visual axis, then if the point happened to be coincident with the geometrical centre it was an easy matter with the aid of callipers to place the optical centre there

or if necessary, to place it in any other stipulated position.

He would like to know where they should be placed in the following combination: R+1.0 L−2.0 Reading Addition +1.0.

It being borne in mind that in the case of fused bifocals with continuous exterior surfaces it was a physical impossibility to centre both distance lenses and both reading lenses. It was usual to place the distance centres there (illustrating) coincident with the geometrical centres and 2 mm. above the segment, but they could be placed in any other position, and it might be advisable to do so, because with the present practice the whole prismatic error which this form of bifocal introduced with such combinations was in the reading portions. Assuming that a total difference in deviation of 3 degrees of angle was introduced, he would suggest that the optical centres be so arranged as to split the error up between the reading and distance lenses. If there was some doubt as to whether this was advisable, it might be split up in a definite ratio according to the power of the distance and reading lenses.

This was a subject which had not received much attention in the past, and although he had brought the point up on at least two occasions, he had never been able to get a general concensus of opinion as to what should be done under such circumstances. In practice he always placed the distance centres 2 mm. above the segment unless orders were given to the contrary. Taking an extreme case of a minus 9.0

diopter distance lens with an addition of two, making minus 7 for reading, a prismatic effect of 7 or 8 prism diopters was introduced, which was certainly in excess of the permissible amount. He would rather like to hear an authoritative statement as to what was considered the average maximum amount of deviation permissible between the two visual axes in the vertical plane. Supposing that for distant work the right eye deviated up 1 degree while the left eye was deviated down 1 degree, was this a permissible amount, or could they go beyond it? Or was it advisable to use separate lenses for distance and reading in these cases, or bifocal lenses of a form which would permit of accurate centering of each of the two portions for the particular combination? Also what was the limit of inaccuracy for the reading lenses? Should it be the same as for distance vision, where the convergence was approximately nil if the correction were exact? He had never seen any data on the point, and so far as his experience went he was inclined to think that many people got used to a variation of 2 or 3 degrees without noticing any fatigue symptoms. With regard to the effect of the visual change from the distance to the reading portion and consequent fatigue, he would point out that in this form of bifocal, in which the junction was perfectly invisible, the intensity of the image was approximately the same, there was not that fatigue felt as with those forms of bifocals which had a distinct mark, thereby causing a shadow to pass over the retina every time the eye was diverted from one portion to the other.

DR. CLAY asked if Mr. Mackinney meant that when they were looking through the upper part of the lens the one eye was deviated 3 degrees more than the other as compared with when they were looking through the lower part of the lens.

MR. MACKINNEY explained that there was a vertical deviation of one eye as compared with the other, and that the error had either to be in

the distance portion or the reading portion, or divided between them.

MR. DIXEY said they could not make them parallel for the distance portion. If they had a plus one and a minus one the only point where they could get their visual lines would be the exact geometric centre. As soon as they erred 1 mm. above it or below it, then their visual lines were out of parallel.

MR. MACKINNEY said what he suggested was that the visual lines should be out of parallel to a given permissible amount under such circumstances.

MR. DIXEY said there was no definite degree of deviation. It varied right away from the centre to the margin of the lens.

MR. MACKINNEY said he took it the patient looked through the optical centres, and he did not get deviation for distance in the vertical plane under present practice, but had the whole error in the reading portions.

DR. CLAY said it was impossible to get them both right.

MR. MACKINNEY said he knew that it was impossible in the particular form of lens. It was quite clear from the fact that the two exterior surfaces of the lens were continuous. He wanted to know, however, in what proportion to divide the error between the distance and reading portions.

MR. HUGHES said he had often noticed—he had a good many centres to take with binocular glasses—that many men would use a wider centre—would be liable to use a wider distance in the centres of two objects in a binocular than others. Most of the cases he had noticed—he had not got their refractive conditions, so that he did not know what bearing they had on the point—but in most of the cases he had always thought that a very important factor in determining the centres of spherical lenses was the muscular balance. He thought in order to get anything like an exact distance between the

centres for a pair of spectacle lenses, one ought to know the muscular condition exactly. He noticed no one had referred to that in the discussion. He was very much surprised to hear that it made 5 mm. difference in the reading. In his own case he used that small instrument on the table with the two pinholes, and he found that between the distance and reading centres according to that 3 mm. at the outside was his own difference. It might have been his want of practice in observing with it, but it seemed to him that that type of instrument would not be suitable for use in an adjusting room, as the difficulty would be with the different people who observed with it; they would get such different results. He had always used, as far as he could get it, the exact pupillary distance between the centres for distance, and allowed 3 mm. for reading, and he had never had a case in which by altering the centres he had found any advantage.

Mr. Chalmers said on the question which Mr. Mackinney raised—the possibility of sharing the prismatic effect between the distance and the reading condition, he thought one thing should be taken into account, and that was that in the great majority of cases there was a larger amount of distance work than near work. He thought it was a very great mistake to bring into the question any appreciable amount of divergence for constant wear for distant objects. He thought it would mean that fatigue would be very much greater. In any case if they were going to split up the deviation between the two parts—between the distance portion and the reading portion—they must give the distance portion as much advantage as they possibly could. He thought it would be desirable in cases of that kind if they could have some sort of terminology for use in discussion, which would define what was really not quite a geometric centre.

Mr. Dixey said he thought it was for Mr.

Chalmers in his authoritative position to give them a term "ex cathedra," so to speak, which would define the centre of a lens.

MR. CHALMERS said that in dealing with the optical centre, as he understood it, it was any point within a lens where the light passed through without deviating.

MR. DIXEY said that did not meet the case of the centering of spectacles. They should get something of the relation of the lenses to the visual lines.

MR. CHALMERS said the two lenses of the spectacles were so arranged that the difference between their centres exceeded or was less than the distance between the two visual lines in plano spectacles by a certain amount, and the error in distance was the error in centering. He thought they must define the optical centre of each lens separately, then they could define the error in centering. The theoretical value should be determined differently for reading and distance vision. There was one statement he would like to make in explanation of some difficulties experienced. Mr. Dixey had called attention to the fact that there was a difference between the proper positions of the lenses for centres as much as 5 or 6 mm.

MR. DIXEY said it depended on the reading distance.

MR. CHALMERS said that the approximate standard value was about 5 or 6 mm. for an adult. He had often heard people say that it was possible to make a reading lens with 2 mm. decentration. That ought to be 2 mm. in each lens. In a great many lenses the reading lens was made 2 mm. smaller than the interpupillary distance. That might account for the fact that many people said it should be 3 mm. when it really should amount to from 5 to 6 mm. He had had that difficulty in actual class work, and he thought it might be possible to explain what were apparently conflicting statements. He thought that in ordinary prac-

tice the difference was usually 4 to 6 mm. between the distance and the reading centres.

Mr. Reiner said that Mr. Hughes had mentioned binoculars, and he was rather inclined to think that the effect mentioned was due to the increased stereoscopic effect; but anyone using a binocular would rather prefer to exceed the interpupillary distance than otherwise. The problem that Mr. Mackinney raised was one that required a lot of consideration. It was a very puzzling point. He did not think any bifocal lens was going to produce a ratio which would be constant for the two lenses. He did not think it possible at all. He had seen a good many made with prismatic segments. An attempt had been made, but they could only keep it within the one lens. They could produce a centre at two points, or at more points, but the result was that the section of the lens would be something of that kind (describing). He did not think anyone would ever make one which would get over that difficulty, to produce one centre there and one there (describing) to produce the same ratio in the lower lens. He did not think it was possible at all. That rather raised the question how much induced hyperphoria one could stand. It was probably only 1 degree. Probably a great deal of discomfort would be caused if a larger amount were induced. Mr. Hughes also raised the question of muscular condition; but he thought everyone knew that the question of the effect of prisms was a very debatable point, and really it was not within the province of the refractionist, and could hardly be dealt with in that condition. With regard to Mr. Dixey's point, he still maintained that he was not getting a proper effect when he looked down through the glasses; although he had got a plane glass there he had still a prism when he looked down. It did not actually produce a disturbance. With regard to the relation between the two eyes, it produced a difference in the poise of the head.

MR. DIXEY said he did not think it was quite right to take it as a fixed law that all minus lenses should be centered beyond the actual visual centres, and all plus lenses centered inside the visual centres.

MR. CHALMERS said he knew it was a general rule, but he did not think it was right. If they had the means of knowing the interpupillary distance he thought they should work to that and not to any point beyond it or above it.

MR. DIXEY: The interpupillary distance was not a fixed value. When they talked about distance, it might mean infinitely far and it might mean anything up to half a dozen yards from one, and the amount of convergence, and therefore the interpupillary distance, varied between those two points.

MR. REINER said he was speaking of the distance known as the base-line, which was that point between the centres of rotation, and if they could get an instrument which would measure that distance between the centres of rotation it was simply a matter of gauging from that what was the correct amount for reading at a known distance. The only unknown factor, he thought, was the distance between the centre of rotation and the plane of the lens. If they knew that it was a very simple matter to get the distance on the lens plane.

DR. CLAY said that a patient, whether myopic or hypermetropic, when fitted with his proper correction, should have a normal range of accommodation corresponding to his age, and should be able to see objects distinctly at any distance within that range. The convergence he would have to exert would, of course, vary with the distance of the object upon which he fixed his attention. He took it that Mr. Dixey was suggesting that one device would be better if convex lenses were required and another device if concave ones were used, so that his convergence might be assisted.

MR. REINER said he thought Mr. Dixey was

working on the line of the plane of a lens, and he (Mr. Reiner) was working on distance apart of centres of rotation, which did not alter. He thought Mr. Dixey wished to express that there were two centering points, one in each lens of equivalent prismatic power. Perhaps that would be a definition that would meet it. It was rather difficult to express two centres there, because there must be a certain relationship between those centres and the distance centre. In the movement of the eyes over that particular lens it was very difficult. They could not produce the centre which was going to be exactly in the same place, but he thought a point was trying to be made of having a certain spot which would have the same prismatic power as an equivalent spot there (indicating).

Dr. Clay said if there were several lenses the light from more than one of them would enter the pupil of the eye at the same time, and they would see at the same time two images which would lead to a certain amount of confusion, just as when they held the bifocal and looked through the dividing line. If they had more than about three there would not be a sufficiently clear space for the pupil.

Mr. Reiner said the same thing might apply to three lenses.

Dr. Clay said that one point Mr. Mackinney raised about the centres was necessarily a difficult one. If a patient required a convex lens in front of one eye and a concave in front of the other, supposing the lenses to be properly centered for one direction, when the patient looked either above or below this direction, prismatic effect would arise, as the one lens would act as a prism base up and the other as a prism base down, and one eye would have to be turned more than the other to retain monocular vision. The same thing would occur when the patient looked to the right or to the left, but as the musoles which affect the convergence were much more under control, this was less important than

it was in a vertical direction. Where most people find very great difficulty in moving the eyes independently, obviously the extent of the prismatic effect became greater the further from the optical centres where the points of the direction of vision out the lenses.

Mr. Chalmers said he thought one thing had been overlooked—*i.e.*, that it was very desirable to be able to move the eyes a little sideways, in which case they would come on the edge. That was why he thought it was desirable to make the distance portions agree rather than the portions for reading, because the eyes were used more for distance than for reading.

The proceedings then terminated.

THE THOMAS YOUNG ORATION.

By Prof. R. W. Wood (of the Johns Hopkins University, Baltimore).

Lecture delivered on 29th September, 1910.

It was not without some misgivings, and only after a good deal of hesitation that I decided to take as the subject of this lecture a special line of investigation upon which I have been engaged, which, while it has to do with the development of optical instruments, and may be of interest to you, is scarcely broad enough to be made the subject of an oration.

We may regard the work of Thomas Young as having laid the foundation for a class of optical instruments which are of the greatest importance at the present time. I refer to the modern interference spectroscopes. We have at the present time three optical instruments, which, by reason of their wide fields of application and the enormous number of new facts ascertained through their use, stand in a class by themselves: we may, in fact, say that we have but three optical instruments of importance, the telescope, the microscope, and the spectroscope.

We have about reached the limit of magnifying power in the case of the telescope, and it seems very doubtful whether we may expect to see much more of planetary detail with larger instruments, for the earth's atmosphere, even under the best conditions, interferes with our vision through a large telescope, in much the same way as does a piece of bad window glass under ordinary conditions. The same is true in the case of the microscope, though in this case it is the finiteness of the wave-length of light which sets a limit to the resolving power of the instrument. In the case of the spectroscope there

appears to be no theoretical limit to the resolving power, and it is probable that improvements will go on for some time to come.

It is my intention this evening to deal both with the telescope and the spectroscope, and bring to your attention some recent experiments which I have made with a view of improving the latter, and perhaps degenerating the former. There are at the present time three types of dispersing systems utilised in the spectroscope; the prism, first carefully studied by Sir Isaac Newton, whose work may be regarded as the foundation on which the spectroscope was built; the diffraction grating, the out-growth of the discovery of the interference of light by Thomas Young; and the more recent interferometers, echelons, and interference plates of Michelson, Fabry, and Perot, and Lummer and Ghercke, of which Dr. Young may also be regarded as a remote ancestor.

It appears probable at the present time that further improvements will most likely be made by increasing the size and perfection of diffraction gratings; for they have already far surpassed the prism in power, and are free from the many disadvantages inherent in most of the interference types.

One great objection to the use of the grating has been the large loss of light which results from the circumstance that several spectra of greater or less intensity are always produced by it.

The diffraction grating, in the form in which it is now used, consists of a polished plate of speculum metal, either plane or concave, upon the surface of which parallel equidistant lines have been ruled with a diamond point to the number of fifteen or twenty thousand to the inch. When a ray of white light falls normally upon such a plate, the rays are bent or diffracted to the right and left, forming a number of coloured spectra, such as I now show you by reflecting the light from this lantern upon the

ceiling. A prism acts in much the same way, only in this case but a single spectrum is formed. Now, it is found in practice that the distribution of the light among the different spectra formed by a grating is by no means constant, but varies with the form of the diamond point used in the ruling. Some gratings have been ruled which throw as much as 50 per cent. of the light into a single spectrum. Such gratings as a rule are produced by accident, for very little has been accomplished thus far by grinding the diamond point to any special form.

We know in general the form of groove which we require to send most of the light into a single spectrum, but very little experimental work has been done on the relation existing between the form of the groove, and the distribution of the light among the different spectra. This is a matter of the greatest importance, since the greatest drawback of the grating is the loss of light which results from the formation of a number of spectra. Very little experimental work has been done upon the subject, since it is almost impossible to ascertain the exact form of the groove in any particular case. Anyone who has worked with the microscope will realise the difficulty of telling anything about the shape of the cross section of a series of fine scratches ruled upon a polished metal surface.

It occurred to me that a promising method of attack would be to manufacture gratings with grooves of such large size that the determination of their exact form, width, etc., would be a matter of certainty, and then investigate the distribution of the energy of the reflected radiation among the different spectra, by means of the very long waves of heat discovered by Rubens and his collaborators. If the light of a Welsbach lamp is reflected from several surfaces of quartz in succession, the energy reflected from the last surface is made up of a very homogeneous radiation, made up of waves, the mean wave-length of which is about fourteen times

greater than the longest visible waves (red light).

By employing these residual rays, as they are called, and a grating with 1,000 lines to the inch, we should have about the same ratio of wave-length to grating space as obtains in the case of a Rowland grating with 14,000 lines to the inch, and red light. Gratings with constants varying from 0.1 mm. to .01 mm. could be studied by means of residual rays, or narrow regions of the infra-red spectrum, isolated by a salt-prism spectrometer, and the relation between the intensity distribution and the form of the groove determined. Methods have been worked out by which a groove of any desired form could be ruled, with optically flat sides (a very important point), the angular slope of each side of the groove measured, and the exact nature of the ruling determined—*i.e.*, whether the metal had been forced up between the grooves, or whether the angle between the opposed faces was equal to the angle between the edges of the ruling knife. This by no means follows, as the ruling of groove No. 2 may force the metal to one side and increase the angle of slope of the adjacent side of groove No. 1.

Gratings were finally obtained, which have proved so efficient in the investigation of infra-red spectra that it seems worth while to designate them by a name of their own. They throw a large percentage of the energy into one or two spectra to the left of the central image and show little or no trace of any energy to the right of it. With visible light they send the greater part into a group of spectra, say, from the 12th to the 16th, or from the 24th to the 30th order. They may thus be regarded as reflecting echelons, of comparatively small retardation, and I propose the name "echelette," to distinguish them from the ordinary grating and the Michelson echelon.

Various methods were tried for their production. The first were made by punching the

grooves with a steel die, two adjacent surfaces of which had been ground flat and highly polished. The die was a block of hard steel measuring $3 \times 2 \times 1.5$ cms., and the gratings were punched with an ordinary milling machine, the die being clamped at the proper angle in a fixed position, and a polished plate of some soft metal pushed up against it from below. This method is analogous to the one used by Mr. Thorpe, in making his gratings for the demonstration of predominant spectra, but it did not give very satisfactory results.

After considerable experimenting with various metals and ruling points, I came to the conclusion that soft alloys must be avoided, for it appeared to be impossible to cut a groove with optically flat sides. The crystalline structure of the metal caused the point to rule a groove, the sides of which undulated more or less, causing more or less reflexion in directions parallel to the grooves.

The method finally adopted was the following: A sheet of polished copper plate, such as is used by photo-engravers for the half-tone process, was gold-plated and polished. The plates were found sufficiently flat for the purpose and had a much better optical surface than anything that I was enabled to produce on a copper plate by grinding and polishing, for the final polishing always produced irregular undulations (possibly owing to variations in the hardness), and I was unable to get any suggestions from professional opticians accustomed to the polishing of glass and speculum metal. If anyone has worked out a method of getting a flat optical surface on such metals as copper or gold, I shall be very glad to hear of it, as it will doubtless improve the quality of the gratings.

A carborundum crystal was used for the ruling point, and the ruling, in the case of the gratings of very large constant, was done on a small laboratory dividing-engine by hand. This machine had bad periodic errors, and the best

gratings were made with Rowland's first machine, with a 7-tooth and a 15-tooth cam, which gave 2,062 and 962 lines to the inch respectively.

The hexagonal carborundum crystals were selected by breaking up a mass of the substance as it comes from the furnace. Specimens of these iridescent crystalline masses are to be found in most chemical or mineralogical museums. The crystals have the form shown in figure 1, and are mounted as shown in the figure. The natural edges are so straight that they rule a groove with optically perfect sides. Everything depends upon the nature of the edge and the angle at which it is set with respect to

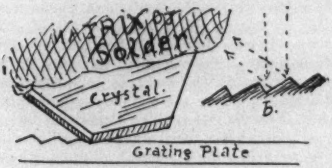

FIG. 1.

the direction of the line—i.e., the tilt forwards or back. Some edges will not rule properly at any angle, "chattering" over the surface and tearing off a thread of metal.

No metal is removed when the ruling is going on properly, the groove being formed by compression of the metal. If the edge is properly chosen, mounted at the proper angle and correctly weighted, a beautiful groove is made with a very little elevation of the metal above the original surface at the edges. The first gratings were ruled on copper and subsequently gold-plated to prevent tarnish, but it was found that even the lightest polishing on the buffing wheel destroyed the sharpness of the edges and caused the development of strong central images.

It was found, however, that even with an ex-

ceedingly thin deposit of gold (about the lightest plating ever done commercially) it was possible to rule very deep grooves without uncovering the copper. This solved the difficulty, and excellent gratings could be produced at a very small cost. The copper plate was varnished with asphalt on the back to save gold, and gilded in as large pieces as the gilding establishment could handle. These large sheets were then cut up to the required size with a circular saw. They performed fairly well optically, giving almost as good images as an ordinary plate-glass mirror, in spite of the rather rough treatment to which they had been subjected.

It is important to instruct the gilder to do as little buffing as possible. My first plates were spoiled by having too thick a deposit of gold and too vigorous buffing or burnishing. The best treatment is the one which they give to the thinnest coats, which would be completely removed if polished by the methods employed for thicker deposits. If a thick deposit is given and polished in the usual way, the optical surface is ruined by the formation of undulations, though it is hard to convince the gilder that it is unsatisfactory. I mention these details for the benefit of others undertaking the manufacture of these gratings, for it took me nearly a week to convince the gilder that he could be taught anything about the nature of metals and how they should be treated.

To obtain a better optical surface, or rather a flatter one, I had a polished flat plate of speculum metal, such as is used for making Rowland gratings, silver-plated and polished. The circumstance that the first plate which I placed in the hands of the plater flew into three pieces as soon as he put it in the hot alkaline solution which they use for cleaning thin metal work, convinced him that there was something about metals to be learned, and he was more willing to take advice thereafter. By this method it was possible to get a beautiful optical surface of

soft metal, in which the grooves could be cut. So far as I could see by a rather superficial examination, the optical perfection of the surface had not been materially affected.

It seems probable that this method will be a distinct advance in the manufacture of gratings for optical purposes. Not only can a larger concentration of light in a single spectrum be secured, but the ruling point does not wear away as it does on speculum metal. We may, moreover, choose our metal with respect to the part of the spectrum in which we wish to work. A gold-plated surface would make an excellent grating for working in the region between wavelengths 55 and 75, for gold reflects 90 per cent. of red light, while speculum metal reflects only 65 per cent. Nickel would be better for the blue and ultra-violet.

The angle of the ruling edges of the carborundum hexagonal plates is 120°, consequently the sides of the groove make approximately this angle. By placing the crystal in various positions we obtain grooves of various shapes, one side, for example, sloping at an angle of 12° with the original surface, the other at 48°. These angles are subsequently determined with a small spectrometer or by simply mounting the gratings on a graduated circle, and observing the reflexion of a lamp-flame in them. In the best ones no trace of the central image can be seen, which is what we should expect if the edges of the grooves were sharp and none of the original plane surface remained. The sum of the two angles of slope did not always add up to 60°, as they should do if the ruling had been done with a 120° point. This is probably due to the circumstance that the edges of the carborundum crystals are usually bevelled as shown in fig. 1. I have not made a study of the angles at which these small planes meet, but it seems likely that with certain crystals we may have a ruling point the edges of which meet at an angle larger than 120°.

Of the eight gratings which I have measured thus far, the angles of the edges and their sum are shown in the following table:—

11	20.5	22	29	11	12	17	43
49	27.5	30	18	44	46	37	11
60	48	52	47	55	58	54	54

In addition to knowing the angle of slope of the two sides of the groove it is necessary to determine whether they make a sharp angle, *i.e.*, whether they meet in a knife-edge at the top, or whether there is some of the original surface remaining between them, or a ridge of more or less roughed surface due to the squeezing up of metal by the compression resulting from the action of the carborundum crystal.

Some difficulty was found in interpreting the appearance of the surface under the microscope until the following method, which gave beautiful results, was tried. Two electric lamps were placed just above the stage of the microscope, to the right and left of the tube, in such positions that the edges of the grooves reflected light vertically into the objective. A red glass was placed in front of one lamp, and a green glass in front of the other. The edges of the grooves appeared brilliantly illuminated in complementary colours, with no dark region, if they met at the top, but if not, each pair of red-green strips was separated from the neighbouring pair by a dark line, due to the fact that the level surface between the grooves was not at the proper angle to reflect light from either lamp into the objective. The appearance of the gratings illuminated in this way was similar to that of one of the screens used in the Joly process of colour-photography, as I shall show you at the close of the hour.

If the grating showed no central image with light, it was safe to assume that the metal along the dark line had been forced up and the original surface destroyed. This was usually

the case when the grooves were very nearly in contact. If strong central images were exhibited, it indicated that a portion of the original flat surface remained between the grooves. The width of this portion in comparison with that of the roughened portion could be determined by placing the grating at an angle under the microscope and reflecting light from the linear strips between the grooves. In this way a very perfect knowledge of the exact nature of the ruled surface could be obtained. Another method of studying the surface is to make a cast of it in celluloid or a paraffin composition and section this with a microtome. The optical method gave the best results, however.

In the majority of cases the crystal was mounted so as to rule a groove one edge of which made an angle of 20°, or less with the original surface. With normal incidence this gives us a concentration of energy at an angle of 40°, with practically no energy thrown off from the other edges of the groove, owing to the steepness of the angle. This case is shown in fig. 1 *b*. The best gratings show no reflexion in the normal direction, *i.e.*, they give no central image. They give, however, a very good reflected image of one's face, when held at an angle of 20°, the image being uncoloured, but slightly diffused by diffraction in a direction perpendicular to the grooves. The image is so sharp, however, that the pupil of the eye can be seen without difficulty.

Some of the gratings with symmetrical grooves show two oblique images. Mirrors made in this way could be utilised by two people wishing to shave simultaneously! Each person would see only his own face in the mirror. We have here a photograph of one of the gratings in front of which is a piece of thin paper with the letters A B C and D marked on it. A strong light falls on the paper from behind, and the letters consequently show through. Notice now that in the

unruled portion of the grating the strip of paper is seen reflected a little to the right of the position occupied by the strip. The intermediate portion of the strip with the letters B and C is seen reflected much further along to the right, at the very end of the ruled portion of the plate. At the end of the hour you will have an opportunity of examining these curious oblique images reflected from the ruled surfaces of the echelette gratings.

The gratings behave, with infra-red radiation of wave-lengths, above, say 3 μ, precisely as an ideally perfect grating, that is, they give spec-

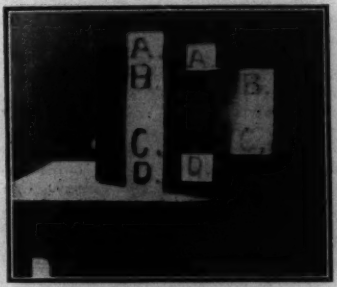

FIG. 2.

tra similar to what we should have with an ordinary grating which threw practically all of the light into one or two orders on one side of the central image.

With visible light their behaviour is most curious and interesting. The central image is usually absent, and we get a blaze of light when the grating is turned at the proper angle. With a symmetrical groove the blaze is seen on both sides, at angles of 45° for normal incidence. This blaze we may term the oblique image.

If the source of light is white, a lamp flame,

for example, the appearance is as shown in fig. 3 *d*, which is for a grating with a constant of .05 mm. The position of the central image is indicated by an arrow. It is very faint or barely visible, however. To the right and left are the oblique images, but very slightly broadened by diffraction owing to the width of the reflecting edges, which in this case make equal angles with the surface.

In fig. 3 *a* we have the appearance of things with a grating of constant .0123 mm. The central image is indicated by an arrow, and is bordered on each side by the ordinary grating spectra, which are close together on account of the coarseness of the ruling. They are much fainter than I have indicated on the plate. Well to one side, at an angle of about 40° with the normal, we see a very bright and greatly broadened white image of the flame, accompanied by lateral spectra.

These are not grating spectra, but the first class spectra (as Fraunhofer termed them) due to a single slit, or in this case to a single reflecting edge of a groove. With a sodium flame the appearance is as shown in fig. 3 *b*. We have in this case three orders of spectra in the region occupied by the central maximum of the spectra of the first class. Their order is indicated below. One or two orders to the right and left of this group are absent, since they fall in the region of the minimum due to a single slit. They are the "absent spectra" of grating theory. Other groups of orders appear in the regions occupied by the first class coloured spectra, their intensity being much less, however, than the intensity of the ones falling within the region of the central maximum.

The existence of these images of the soda flame shows us that the perfection of ruling is such that interference, with a path difference of about 30 wave-lengths, is still taking place. In other words, our grating is acting as a reflecting echelon with steps 15 wave-lengths in

height. This was observed only in the case of the gratings ruled on the Rowland machine. Those ruled on the small laboratory machine by hand showed only a confused jumble of overlapping images, which formed an almost continuous band of yellow light. The grating constant was .0123 mm. in the case represented in figs. 1 and 3. With a larger constant the central maximum of the spectra of the first class was narrower and brighter, the lateral rainbow coloured fringes being less in evidence.

The width of the region in which we have these maxima and minima of the first class be-

FIG. 3.

comes less as we increase the width of the reflecting steps of the grating. In the case of a grating of such a small constant as .0123 mm., this region of diffraction of the energy from each individual element covers a range of fully ten degrees, embracing as many as 12 or more orders of second class spectra. In the case of the Michelson echelon, the width of the step is from 0.5 to 1 mm., and the range of diffraction is so small that but one or two orders of spectra are included within it. By the study of these echelette gratings we can pass gradually from

the case of the ordinary grating to that of the echelon.

Some of the gratings, with a constant of .0123 mm. gave strongly coloured central images, and lateral spectra of low order in which a certain colour or colours were wholly absent. The first order spectrum on one side, for example, may contain no yellow-green, a broad dark band bisecting the spectrum. A third order spectrum may have two dark bands, one in the yellow, and another in the greenish-blue.

In one case the oblique image or the central maximum of the spectra of the first class, instead of being white, was distinctly blue, while the maxima immediately to the right and left of it contained only red, orange, and yellow light, as shown in fig. 3c.

This curious distribution of colour was observed in the case of one grating only, and its explanation gave a good deal of trouble. To explain it we must devise some type of reflecting element which will give, in the case of red light, zero illumination at the centre of symmetry, with strong lateral maxima, and with blue light a strong maximum at the centre, bordered by minima which occupy the positions of the red maxima, and maxima in the positions of the red minima.

This can apparently be brought about only by an element consisting of two parts, in other words a double reflecting strip, with a half-wave retardation for red light, as in the case of the laminary grating. The central maximum will vanish in this case for red light, as can be easily seen by constructing the diffracted wave-fronts. If blue light is used the retardation becomes very nearly a whole wave, and we have the centre of the system bright. An examination of the grating with a microscope showed that there were in fact two reflecting strips in contact which together formed one side of the groove, the grating being built up of paired reflecting elements separated by inopera-

tive strips of about the same width. Just how the carborundum crystal managed to rule such a groove I am unable to say. A coloured drawing of this very remarkable set of spectra will be found in my "Physical Optics." (Revised edition, 1911.)

Further experimenting will probably improve the quality of the gratings and open up a large field of work in the infra-red region. The gratings yield excellent replicas which can be mounted on heat plates of glass and gold-plated by the cathode discharge. The replicas will very possibly have flatter surfaces than the original gratings, if properly mounted.

I have described in some detail the appearances presented by these gratings when examined with visible light because they are in a way quite unique, and their study has proved very instructive. We will now take up their behaviour with heat waves fifteen times longer than the light waves. The investigation of this subject I have carried out in collaboration with Professor A. Trowbridge, of Princeton University.

The investigation was made with the large vacuum spectro-bolometer, on the table of which the gratings were mounted, and the slit illuminated with the radiant energy in question. Two groups of rays were used in the investigation, the residual rays from quartz with a mean wave-length of 8.6μ and the CO_2 radiation from a Bunsen flame, with a wave-length of 4.3μ, or about half as great as that of the quartz rays. The smallest grating constant used was 0.0123 mm., or seven times the width of the grooves on the gratings ruled on Rowland's first machine, the largest, .05 mm. It is evident that when these gratings are used with the long heat-waves above referred to, the ratio of the grating constant to the wave-length is about the same as that which obtains in the case of visible light and the optical gratings in common use.

In order to make a thoroughly satisfactory

study of the distribution of the energy it would be advisable to keep the angle of incidence fixed (for example normal) and swing the bolometer or thermopile through the spectra. With the instrument at our disposal at the present time this was impossible, and it was necessary to make the spectra pass across the bolometer by rotating the grating. This complicates the discussion of the results in no small degree, for the energy distribution varies with the angle of incidence, as can be seen easily with an ordinary grating. We have, however, already obtained results which are in qualitative agreement with theory, and which show that the me-

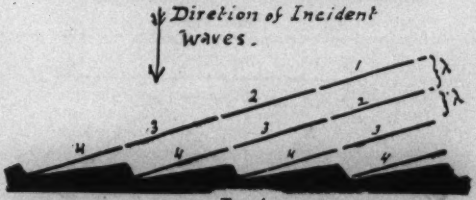

FIG. 4.

thod is admirably adapted to the experimental investigation of the problem. We shall, in the present treatment, discuss the results by the Fresnel method, considering the interference between secondary wavelets originating on the surface of the wave-fronts reflected from the oblique edges of the grooves. As Lord Rayleigh has pointed out, this method holds only when the width of the groove considerably exceeds the wave-length of the light.

In the present case, with our closest ruling, the groove-width was 1.5 times the wave-length of our longest waves, and it appears probable that in this case we are very near, if not beyond the point, at which we may safely employ the Fresnel treatment.

In continuing the work it is our intention to employ waves of continually increasing wave-length, until the point is reached at which the spectra disappear entirely, which will give us the complete experimental solution of each case.

In the case of the echelette grating the conditions are quite different from those which obtain in the case of the gratings usually considered which act by opacity. For a wire grating, or a reflecting grating made by ruling black lines on a reflecting surface, the spectra of even order fall out when the widths of the operative and inoperative elements are equal. In the case of the echelette grating, practically the

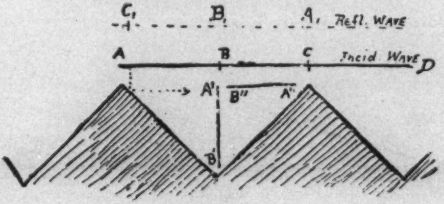

FIG. 5.

whole surface is operative, and if we place the eye, or better the objective of a microscope (focused upon the grooves in the direction of a spectrum) we see a uniform blaze of light illuminating the entire surface. This means that the widths of the reflected elements of the wave-front are twice as wide as in the case of a grating of the opaque type having the same constant.

Now in a grating of this type the spectra of even order disappear when $a = b$, as a result of the circumstance that in the directions of these spectra each diffracted wave front is *self-destructive*, *i.e.*, these directions are the directions of Fraunhofer's minima of the first class, namely such as will make the path differ-

ence between the disturbances coming from the two edges of each reflecting element equal to the wave-length of light. In the case of the reflecting grating with its opaque strips, if we widen the reflecting strips and narrow the opaque ones, keeping the constant the same, the direction of the first class minima will move in towards the first order spectra, which will disappear when the opaque strips become infinitely narrow. The same thing, however, holds for all the other spectra, for as we widen the reflecting strip the first class minima draw closer together, coinciding with the spectra of the second class (grating spectra) in the limiting case of opaque lines infinitely narrow. If, however, we *narrow* the reflecting strips, keeping the grating space constant, the first class minima move out and presently the spectra of the third and sixth order disappear.

Going back now to the echelette grating we find that in the ideal case, in which the reflected fronts build up an unbroken surface (*i.e.*, with no inoperative or dark regions between them) we should expect all of the light in one spectrum, namely the one lying in the direction in which the reflected wave-fronts are travelling, the case being analogous to the reflecting grating with infinitely narrow opaque lines, except that in this case we find the light in a spectrum instead of in the central image. We must remember, however, that in this case we have chopped up the wave-front into linear strips, and that our reflected wave-front is built up of strips obtained from successive waves, as can be seen from fig. 4, in which we have the reflection of a train of four waves, numbered 1, 2, 3, and 4 from the echelette grooves. It is very questionable whether the upper wave-front 4, 3, 2, 1, will behave as a plane-wave, *i.e.*, travel out without diffraction, for each one of the elements of which it is composed has had to travel one or more wave-lengths before uniting with its neighbour.

This is a question, however, which can be best answered by experiment. In the case figured the reflected waves are travelling in the direction of the first order spectrum, and the path-difference between the successive elementary wave-fronts is λ.

Neglecting the probable disturbances in phase continuity resulting from the breaking up of the wave into narrow strips, we should expect all of the energy in the first order spectrum. If, however, we work with waves twice as long, the path-difference will be $\lambda/2$ instead of λ and we should find the energy about equally divided between the central image and the first order spectrum, which in this case will lie well to the left of the direction in which the reflected waves start. Grating No. 8, which will be described presently, comes the nearest to fulfilling these conditions of any thus far examined. With waves 4.3μ in length the first order spectrum lies nearly in the direction of the reflected waves, and contains 70 per cent. of the energy. With the "Reststrahlen" from quartz ($\lambda = 8.6$) we have 34 per cent. in the first order spectrum and 66 per cent. in the central image. The preponderance in the central image is due to the fact that the "oblique image" (direction of reflexion) lies nearer to the central image than the first order spectrum for 8.6μ.

A large number of gratings have been examined and the work is not yet completed. For a complete solution of each case, it is necessary to know whether any of the original flat surface has been left between the grooves. This is often the case with the coarser rulings, and results in the formation of strong central images when the gratings are examined with visible light. Each grating element may thus consist of three strips, the two edges of the groove and the flat portion between. Thus far, but a single type of groove has been tried, viz., the one ruled by the 120° carborundum crystal. The angle at which the crystal was mounted with respect to the sur-

face has, however, been varied over a wide range, as well as the depth of the groove, &c. Other types of grooves will be investigated with a view of finding the one best suited for work in the infra-red. It seems probable that a 90° groove will be the best, as with a groove of this type one

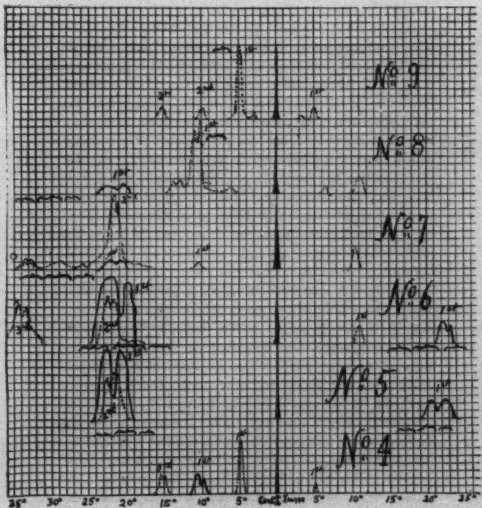

FIG. 6.

edge can be made almost inoperative, and a larger proportion of the surface brought into play. A symmetrically placed 90° groove with the light incident normally will be an interesting type to investigate, for in this case we have a two-fold reflexion in the groove, each element

of the plane wave being broken into two, which are turned end to end and reunited, as can be seen by constructing the reflected rays for a surface of this nature. A 90° double mirror has the property of returning a twice-reflected ray back to its source, regardless of its direction, provided it cross the groove in a direction perpendicular to the groove.

In fig. 5 let AB be a portion of the plane wave AD incident upon the grooved surface. After the first reflexion it will occupy the position $A'B'$, and after the second $A''B''$. The portion BC will be reversed in the same way, and the two portions will unite into the wave $C_1B_1A_1$. It seems, therefore, as if a surface of this nature would not interfere with the constancy of the phase along the wave-front, notwithstanding the fact that the wave has been chopped to pieces, and the pieces made to change places. This being the case, it appears as if we should have no diffraction spectra at all, in spite of the deep furrows.

Just how a surface ruled with grooves of this type, with perfectly smooth reflecting sides meeting in a sharp edge, would behave is perhaps open to question. Whether a wave can be broken up into paired strips, reversed, and reunited into a plane wave without suffering diffraction, is a question which can probably be answered only by experiment. It seems possible that many of the anomalies exhibited by reflecting gratings can be explained by a two-fold or even multiple reflexion from the groove. It is doubtful, however, if multiple reflexions can be considered as taking place in a groove commensurable in size with the wave-length. The investigation of gratings of this type will be taken up later. The present paper deals only with the behaviour of the 120° groove.

We will now take up the individual behaviour of the gratings which have been investigated up to the present time.

The arrangement of the apparatus was as

follows: The light from a Nernst filament, rendered parallel by a concave mirror, was reflected from three large polished surfaces of quartz and focused upon the slit of the vacuum spectrobolometer by a second concave mirror. The diffraction spectra were caused to pass across the bolometer strip by revolving the grating, which made the incidence angle vary, and necessitated the use of the formula for fixed telescope and collimator and rotating grating. Each grating was studied with the quartz residual rays, and with the CO_2 radiations, and the curves representing intensity distribution plotted. The areas of the curves were measured with a planimeter to determine the total energy in each spectrum. The "central image" curve was always very much higher than any spectrum curve, but owing to its narrowness frequently contained much less energy; in other words, we cannot take the deflexion at the central image, and in a given spectrum, as a measure of the energy distribution, since the radiation is not monochromatic. The curves were plotted on large sheets of co-ordinate paper, and cannot be reproduced very well, even on a greatly reduced scale. It has seemed best, therefore, to make a small chart, showing in a rather qualitative manner the positions and magnitudes of the spectra of different orders, obtained with each grating. Dotted lines represent the CO_2 radiation (wave-length 4.3), solid lines the quartz rays (wave-length 8.6). To save space the central image curve, the height of which is sometimes 70 or 80 times the width at the base, is shown on a much smaller scale.

The angular position of the oblique image on the chart has been indicated by brackets. In the case of very coarse rulings, it is merely an image of the source, but slightly diffused by diffraction; with the finer rulings we have the broad maxima and minima of the first class (the positions of the superposed patterns due to the individual reflecting strips or the so-called

oblique images, are indicated by brackets in fig. 6).

The gratings have the following constants and characteristics:—

No. 4 Constant (grating space), .0265 mm. Angles of reflecting planes 20.5° and 27.5°.

	Constant.	Angles of Planes.
No. 5	.0123 mm.	22° and 30°
No. 6	.0123 mm.	18° and 29°
No. 7	.0123 mm.	30° and 20°
No. 8	.0123 mm.	8°
No. 9	.0265 mm.	6°

We may take as a type the study of Grating No. 5.

GRATING No. 5. CONSTANT, .0123 MM.

Angle of reflecting planes, 22° and 30°. Microscopical examination showed that the 30° planes were very poor reflectors; they appeared covered with dark patches, showing that the surface was very ragged and there was no oblique image to the right of the central image, which was wholly absent with visible light. With the heat rays the intensity distribution was as follows:—

QUARTZ RAYS.

First order, left	63
„ „ right	7
Central image	30

CO_2 RAYS.

Second order, left	46
Central image	54

In this case the slit width was 0.5 mm., and the width of the bolometer strip was 0.5 mm., so that the resolving power was high. The deflexions of the galvanometer could be repeated with an error of less than 1 mm. (with total deflexions of 35 mm.).

The first order spectrum for the quartz rays falls exactly in the direction of the oblique image, and contains 63 per cent. of the energy. With visible light there is scarcely a trace of any

central image, practically all of the light going into the oblique image, yet with the heat-rays we find the central image quite strong. This is what we should expect, for the heat-rays are diffracted by the edges of the grooves to the same extent as are light-waves by an ordinary optical grating, and the concentration is not complete even when the direction of a spectrum coincides with that of the oblique image.

With the CO_2 rays the first order spectra are absent. This is in agreement with theory, for if we draw the reflected wave-fronts moving off at an angle of 44°, and make their width such as would obtain with a grating constant of .0123 and angles such as specified, we find that if we reduce their width a trifle the path-difference between the disturbances coming from their edges, in the direction of the first order spectrum, is exactly λ.

We should, however, expect the central image to very nearly disappear from the same circumstance, for in this case the path-difference will be 2λ. Its appearance is probably due to the circumstance that the ideal condition of fig. 1 is not fulfilled, *i.e.*, there is a narrow dark region between the reflected wave fronts. We have, moreover, disturbances from the other edges of the groove which have not been taken into account.

The results obtained thus far appear to be in excellent agreement with theory, and indicate that the method gives reliable experimental data regarding the distribution of energy as a function of the groove form. It indicates that diffraction of the radiation from the reflecting planes prevents us from concentrating all of the energy in a single spectrum, but that with a properly sloped edge we can utilise as much as 70 per cent. of the energy.

In the future it is planned to use more homogeneous radiation, by spectral decomposition of white light with a rock-salt apparatus, and study each grating with a wider range of wave-

lengths. This will enable us to pass by gradual stages from the energy distribution obtained with the quartz-rays to that obtained with the CO_2 radiation.

The chief point of interest which has come out of these experiments is that better gratings can be ruled in a soft metal film deposited upon a hard surface which has previously been worked flat, than upon speculum metal. The resolving power of a grating depends upon the total number of lines ruled, and a diamond point will

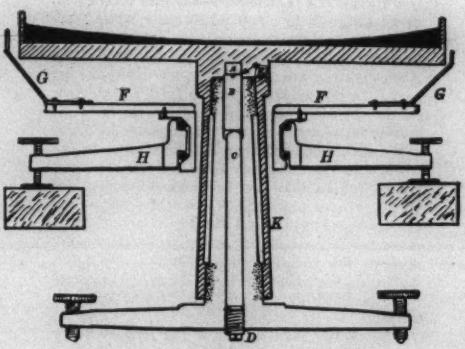

FIG. 7.

seldom rule a five-inch surface without breaking down. On a soft film of gold, silver, or some such metal deposited upon a polished plate of speculum, there is no reason why a grating of any size should not be ruled, for there is very little wear upon the diamond point, and a good point once found should last a long time, instead of breaking down after ruling one or two small gratings.

I shall now change the subject quite abruptly and bring to your attention some experiments

with which I have amused myself during the summer months.

The idea of utilising the principle that the free surface of a liquid, rotating with a uniform velocity, assumes the form of a paraboloid, in the construction of a reflecting telescope is not new. Mercury telescopes have been suggested from time to time for the past half-century, and in the early seventies one was constructed by Mr. R. C. Carrington, on Frensham Common. So far as I know, however, no serious attempt has ever been made to devise a method of rotating the fluid without, at the same time, communicating jars to it. The scheme has always been regarded as a joke by astronomers, which is not surprising when one considers the perfection required of an optical surface and the ease with which ripples are set up on a free surface of mercury.

The idea of using a rotating magnetic field to communicate motion to the circular basin of mercury occurred to me last summer, and becoming interested in the problem, more as a mechanical puzzle than anything else, I determined to ascertain if possible whether, after all, the difficulties were insurmountable. A small instrument seven inches in diameter was constructed and arranged to be driven by a revolving ring of small magnets, which pulled a concentric ring of magnets fastened to the dish around with it. The rotor, as we may term the outer ring, was mounted on a support which was completely insulated from the mercury basin, and the jars which it received from the motor along the driving belt were in this way prevented from reaching the liquid. It was, of course, found impossible to learn much about the instrument in the city, but I convinced myself that the ripples on the surface were no worse when the basin was revolving than when it was at rest.

On removing to East Hampton, L.I., for the summer, where I have fitted up a small labora-

tory, the experiments were commenced in earnest. The mirror was mounted upon a small brick pier and surrounded by an empty barrel to protect it from air currents. It worked so much better than I had expected, in spite of its poor workmanship, that I at once determined to have a larger instrument constructed of the finest workmanship possible. A week or two was spent in experimenting with the small model, in order to learn as much as possible

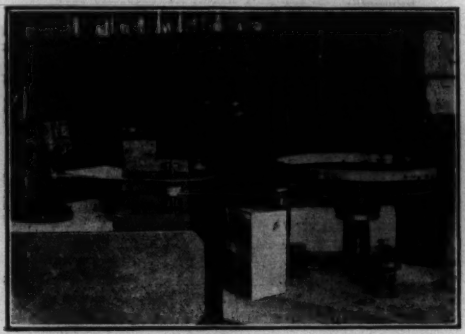

FIG. 8.

about its peculiarities and the sources of trouble, after which drawings for a 20-inch instrument were prepared and submitted to Messrs. Warner and Swasey, of Cleveland.

Mr. Warner very kindly gave his personal attention to the matter, modified the drawings, introducing some new features, and had the work commenced at once. The 7-inch instrument was made from an old casting which I found in the junk heap of the laboratory. It was in the form of a circular disc surmounting

a short cylindrical pillar. The disc was turned out into a shallow flat-bottomed basin, and the pillar bored out with a hole about 1 cm. in diameter, conical at the end. The whole was supported on a steel cylinder, turned to a cone at the top, with a bearing surface about 5 mm. wide at the bottom, the construction adopted in the "turn-tables" made for finishing microscope slides. It was found with this instrument that there were four distinct sources of ripples:

(*a*) Jars from the driving mechanism—*i.e.*, motor, speed-pulleys, etc. These were eliminated by the magnetic clutch previously described.

(*b*) Jars due to the grinding together of the bearing surfaces of the mercury basin itself. These were always present in the first instrument as the surfaces were not polished. It was hoped that better workmanship would eliminate them, as was found to be the case.

(*c*) Imperfect levelling of the instrument, which sets up a sort of tidal wave; easily overcome provided the axis of rotation is accurately perpendicular to the flat bottom of the dish.

(*d*) Variations in the velocity of rotation, which give rise to very troublesome waves. Of these I shall speak more at length presently.

Mr. Warner suggested the use of two slightly tapering conical surfaces for the cylindrical base of the dish and the supporting pillar, with a central shaft of steel, by the elevation of which the dish could be lifted a trifle from the conical supports.

The construction will be readily understood by reference to fig. 7. A plug of hardened steel *A* was inserted in the iron basin, which was screwed to the top of the conical pillar *K*. This plug rested upon a second plug of hardened steel *B*, which could be raised or lowered by means of the screw *C*, turned by means of the nut *D*. The bearing surfaces, indicated by the arrow *E*, were ground flat and accurately perpendicular to the axis of rotation. The weight of the

basin was carried by the steel plug, but there were in addition two conical bearings (dotted in the diagram) which kept the instrument level during rotation. By lowering the screw *C* the weight of the basin could be thrown wholly upon the conical bearings. In this position it could be turned only with some difficulty, owing to the increased friction. The best position was secured by raising the screw by an amount just sufficient to abolish this friction. The flat bearing surfaces have recently been replaced by a steel bicycle ball, which carries the weight of the dish. The friction is much less and the definition of the mirror is much improved.

The driving mechanism or rotor consisted of a large wooden pulley *F*, mounted on ball bearings, carried on a tripod *H*, which was supported independently of the rotating basin. The original plan was to apply the power of the rotor to the dish by means of a system of magnets, but it was found that fine elastic threads of rubber answered equally well. These were fastened to the brackets *G*, six in number, and attached to the rim of the basin. They applied their force tangentially, and did not appear to transmit the jars of the rotor to the heavy iron basin. A photograph of the instrument, the driving pulleys, and the motor, is shown in fig. 8.

Preliminary experiments were made with the instrument mounted on a brick pier in the laboratory before its installation at the bottom of the cement pit. Ripples were always present here, as it was impossible to secure the complete insulation of the rotor, and the jars from the motor were transmitted to the pier through the floor. A good deal was learned, however, about the adjustments of the instrument in spite of the unfavourable conditions. Accurate levelling is of the greatest importance. The dish was first levelled by means of a spirit level, the final adjustments being made optically.

A Nernst lamp, with its filament vertical, was

mounted on the ceiling of the laboratory, at a height of about 12 feet above the mercury surface. At a speed of rotation of about one turn in 3.5 seconds the image was formed at a point where it could be reached easily and examined by standing on a ladder. If a sheet of paper was held a few inches below the focal point, the circular ripples were clearly visible. The bright centre of the ripple system did not, however, coincide with the centre of the convergent cone of rays. By turning the levelling screws it could, however, be brought accurately to the centre which gives us a first approximation to the true level desired. The ripples became much less conspicuous as soon as this operation was performed, and it was speedily discovered that they were in large part due to a sort of tidal wave resulting from imperfect levelling. If the basin is not level the depth of the mercury is slightly greater on the lower side of the basin.

This point of maximum depth is carried around to the high side by the rotation of the dish, and the mercury seeks to establish its level again. If the eye is placed in the focus a curious sort of spiral eddy is seen, which is caused by this continual readjustment of level. With the instrument in approximate adjustment, the surface appears perfectly smooth and free from ripples except under especially favourable illumination. The appearance of the extra-focal image as the level is improved is shown in fig. 9 (upper row). These pictures were made by inclosing a plate in a holder provided with a shutter giving an exposure of about 1-25th second. In the lower row we have images obtained under the same conditions with the plate almost at the focal point.

A star seen under these conditions shows a very marked coma, which can be abolished by turning the levelling screws a proper amount. When the mirror is accurately level, if the image is received on a piece of paper, and one of the levelling screws turned a trifle, the image

moves slightly to one side, just as it would do if we tilted slightly a solid mirror. The image develops a coma, of course, but it is interesting that an actual displacement of the point of light can be obtained in this way, a circumstance which one would hardly have anticipated.

Some experiments have been made with two concentric dishes, the inner floating on the liquid contained in the outer. This expedient does not appear to lessen the effect of jars com-

FIG. 9.

ing from the ground, though it improves matters in some other respects. In the end it may be found necessary, though there are certain difficulties attending its use.

One thing which struck me as especially interesting was the slowness with which the velocity of the dish was communicated to the fluid. The mercury picks up its velocity first at the rim, a circular zone of constant velocity and focus crawling in gradually from the edge toward the centre. If the eye is placed above the centre of the dish a little above a focus, one sees a narrow ring of light of dazzling intensity converge gradually from the rim toward the centre. It required over two minutes for all parts of the

fluid mass to acquire a constant angular velocity.

The appearance of the room as viewed in the mirror while it is getting up speed is very striking. The rafters of the roof recede to an enormous height and the whole room appears to

FIG. 10.

expand in a most remarkable manner. The appearance of the mercury surface in rotation, and its freedom from ripples are shown in fig. 10. The distortion of the reflected image is, of course, due to the astigmatism which results from oblique incidence. If the picture is inverted and viewed from a point only a little above the plane of the paper it appears quite good.

Increasing the speed of rotation shortens the focus, and in the next figure (11) the reflected rays of light have come to a focus before reaching the camera, and the image is seen right side up instead of inverted, as in the first case. In other words, the real image in space has been photographed.

I have had the mirror operating with a focal length of less than three feet, at a velocity of one turn in three seconds—*i.e.*, with a ratio of focus to aperture of less than *F*.2. The dish was not deep enough to make a higher velocity possible, but I have had smaller mirrors running with a focal length of less than one-fourth of the aperture. These very deep paraboloids might prove useful where a great concentration of energy was desirable, as in bolometric determinations of the heat of the stars, or for concentrating solar radiation.

After completing the preliminary experiments in the laboratory preparations were made for mounting the instrument below ground. An old well-house adjoining the barn was transformed into an observatory, a new floor being laid and a large scuttle cut in the roof. The brick well, which was no longer used, was filled up with large granite blocks and Portland cement, to a height of about two feet above the water level, after which it was lined with cement, forming a water-tight pit about 14 feet deep and 30 inches in diameter. The granite and cement bottom formed a very solid foundation for the mirror to stand upon. A second pit was sunk about six feet from the well, just outside of the well-house, and the two put in communication by means of a tunnel at the bottom.

It was the original plan to drive the mirror by means of a pulley-belt running down the side of the cement pit, but as it was found that it was necessary to make very precise adjustments while the mirror was running, the second shaft became necessary. The mirror was mounted

upon three blocks of cast iron, which rested on the cement floor, and the driving mechanism

FIG. 11.

or rotor was carried upon a wooden *T*-beam, which passed under the floor of the tunnel and

was securely anchored to brick piers in the second pit. The electric motor, driven by a 110-volt alternating current, and the speed-pulleys, were mounted in the second pit, the belt passing under the floor just above the *I*-beam and around the wooden pulley of the rotor. The tunnel was lighted by an electric lamp.

The general arrangement of the apparatus is shown in fig. 12. It will be seen from the diagram that the slight jars from the rotor can reach the revolving mirror only by passing back to the second pit along the *I*-beam, thence through the brick piers and back through seven or eight feet of sand to the massive granite foundation. It is needless to say that they were quite imperceptible at the end of their journey. Within the well-house, and close to the mouth of the pit, a vertical wooden cylinder was mounted with a horizontal arm carrying a plate-holder. A photographic plate could, by means of this simple device, be held fixed in any position above the pit, for making star trails. Visual observations were made by standing at the mouth of the pit, or upon the roof of the house, according to the focal length used, and holding an eyepiece in the hand.

It was first necessary to determine to what extent vibrations, other than those caused by the motion of the mirror, affected the mercury surface. A five-inch refractor, with an excellent objective figured by Clark, was mounted at the side of the pit, and directed downward at the mercury surface. The star images were perfectly sharp, even when the motor was driving the rotor, showing that no trouble was to be anticipated from tremors running back along the *I*-beam. The approach of a horse and carriage could be detected, however, when it was an eighth of a mile away, and the footsteps of a person running across the lawn fifty yards from the telescope house caused a perceptible vibration of the image. Occasional disturb-

ances were noticed due to live stock in the barn, and after every storm, vibrations were found resulting from the pounding of the surf on the beach a quarter of a mile distant.

At the time when the experiments were com-

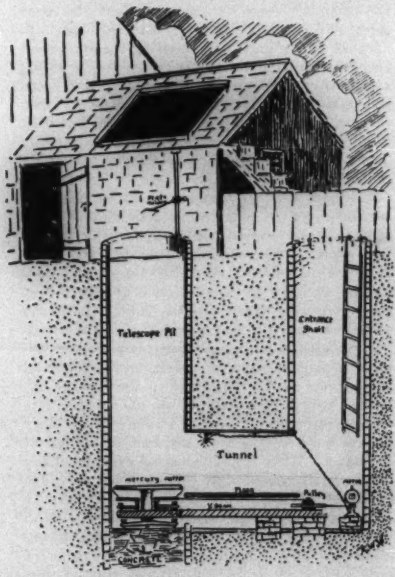

FIG. 12.

menced the Milky Way was in the zenith at nine o'clock, and its appearance when I observed it from the roof of the house the first time the motor was started amply repaid me for all my trouble. No eyepiece was used, the star images

appearing in space about three feet above the mouth of the pit. They appeared perfectly sharp and quite steady, since the cone of light entering the pupil of the eye under these conditions comes from but a small part of the mirror's surface. On examining the images with the eyepiece it was at once seen that the focus changed periodically through a range of perhaps an inch and a half. On viewing the images in space with both eyes they were seen to be rising and falling rhythmically, dancing up and down like will-o'-the-wisps. There were moments when they were quite sharp in the eyepiece, but even then they were not quite stationary, moving about in a sort of Lissajou figure, perhaps a millimeter in diameter.

The nature of the disturbance on the mercury surface could be detected by placing the pupil of the eye in coincidence with the star image. The whole surface of the mirror was then seen filled with a blaze of light and the ripples were plainly visible. It was at once apparent that they were not "jar" ripples, for these always appear as narrow concentric circles surrounding the centre of the mirror. They often appear nearly stationary owing to interference. The ripples which were causing the trouble appeared to start at definite points on the rim and spread out across the surface—*i.e.*, their centres were on the rim and not at centre of the basin. After a good deal of experimenting it was finally found that these ripples were due to slight periodic variations in the velocity which caused a "slip" between the mercury and the iron rim of the basin. At points where there was any roughness, or where grains of sand happened to lie, the ripples started out.

The periodic change of focus was also found to be due to the variation of the velocity, which occurred once in every revolution of the dish. This change in the velocity is very small, and it is not communicated to the fluid, for, as we have seen, two minutes are required for the establish-

ment of the steady state, starting from rest. When the reduction in speed occurs, though it lasts but a fraction of a second, the liquid at the rim falls a trifle, rising at the centre. The propagation of the change of curvature from the rim to the centre is almost instantaneous, and not at all like the slow crawling-in of the zone of constant curvature seen when starting the dish. This trouble was overcome to a great extent by making one or two changes in the construction of the instrument.

Its presence was detected by fastening a small square of paper to the rim of the basin, and another immediately opposite it on one of the supports to which the rubber bands were fastened. If the dish turned at the same velocity as the rotor, the two pieces of paper should remain in juxtaposition: it was found, however, that once in every revolution, the piece of paper on the basin first lagged behind, and then gained upon, the one fastened to the rotor. A little further experimenting showed that this variation was due to the fact that the friction was a little greater at this point, that is, the force necessary to start the basin varied with the position of the dish. When the dish, in its revolution, reached the position of maximum friction, it lagged a little behind the rotor, the rubber threads stretching a trifle. As soon as the point was passed and the friction diminished, the increased tension on the threads caused a slight acceleration, the basin catching up with, and even running a little ahead of, the rotor. The actual slip between the rotor and the basin only amounted to about a centimeter, but this was quite sufficient to account for all of the disturbances, as was shown by intentionally varying the velocity by small amounts.

Resolving Power.

A number of star trails were made with the mirror running with a 15-foot focus. One of these (γ *Cygni*) is reproduced in fig. 9 (lower figure). We obtain a dotted line of bright points,

owing to the periodic change in the focus. The points are fairly small, in fact, not so very much larger than their theoretical size, and when we consider that they were formed by light-rays concentrated by a rotating mercury surface 20 inches in diameter, at a distance of 15 feet from the photographic plate, they appear to me to be surprisingly good. I feel very confident that the velocity variations can be abolished. It may be necessary in the end to use a motor, the speed of which is controlled by a clock, the plan so successfully worked out by Mr. Gerrish of the Harvard Observatory.

FIG. 13.

I had no trouble in separating double stars 5″ or 6″ apart, and I have had momentary glimpses of the resolution of both ϵ^1 and ϵ^2 *Lyrae*.

The great nebula of *Andromeda* and the large cluster in *Hercules* both cross the zenith at East Hampton, and the former object appearing almost as satisfactory as in a large refracting telescope.

I have not as yet convinced myself that the mercury telescope will be of use in astronomical observations even if it is possible to bring it to

a high degree of perfection. Its great disadvantage lies in the fact that its observations are confined to a narrow region surrounding the zenith. On the other hand, it has the great advantage of a focus which may be made to vary over as wide a range as we please, and its cost is very small. The 20-inch instrument, with its motor, cement pit, entrance shaft, tunnel, etc., cost only $200 (about £40), not including the mercury. In the case of small instruments, not over three feet in diameter, it may be feasible to use an auxiliary plane mirror driven by clockwork or by hand, for reflecting the light from any part of the sky to the bottom of the pit. An instrument of large size could in this way be placed within the means of any amateur observer, and valuable data might be secured. Whether a gigantic reflector mounted in a suitable latitude would enable us to see more of planetary detail, I do not feel prepared to say. We could in this way combine great focal length and immense light-gathering power, but we should still have the atmospheric disturbances to contend with.

As I stated at the beginning of the paper, the work was undertaken purely as a diversion for the summer months, and if further experimenting shows us that a perfect reflector can eventually be obtained in this way, it will then be time enough to consider whether it is worth while to attempt the construction of a much larger instrument. Then, too, we may be able to discover some substance which can be fused, rotated, and allowed to solidify while revolving with a constant velocity. Refiguring of the surface would very probably be found necessary.

In the case of a partially viscous substance, there would be much less trouble from ripples. Last summer I made a fairly good mirror of gelatine, and I am at present looking about for a more permanent medium. I shall be very glad to receive any suggestions about easily

fusible media which will solidify with an optical surface.

While it has seemed best to bring the instrument to the highest degree of perfection before attempting to advise methods of destroying any residual ripples, some experiments have already been made in this direction. Two methods suggest themselves. We may use a copper vessel, turned to an approximate paraboloid, and use a thin film of mercury (as in the case of the artificial horizon). The rotation would then have the effect of figuring the surface, the thinness of the mercury layer preventing the propagation of

FIG. 14.

ripples. This method has not yet been tried, as the objections to it are obvious.

The ripples can be damped to an appreciable degree by covering the mercury surface with a thin layer of water, which assumes the same curvature as the mercury. Glycerin damps them completely if they are not of too great amplitude to start with. Even if they are of large magnitude, the effect of the glycerin is surprising. With my seven-inch mirror, mounted on a table which was jarred to such an

extent by the machinery in the room below that the vibrations could be felt with the hand, I have obtained almost as good definition as at East Hampton with every precaution taken to avoid jars.

The action of the glycerin is well illustrated by figs. 13 and 14, which are photographs of the filament of an incandescent lamp taken with the mirror. The mirror was running at such a speed that the image was formed about a meter above the surface. The belt from the motor drove the dish directly, no rotor being used,

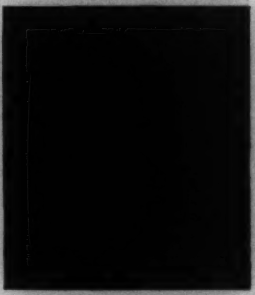

Fig. 15.

and the jars from the city traffic and machinery were so great that the ripples could be seen even in the most unfavourable illumination. Fig. 13 was taken without, fig. 14 with, the glycerin cover.

Two small dishes of mercury were then placed on the floor, which was violently shaken by shafting attached to the floor beams in the shop below. The jarring was so great as to be unpleasant. The mercury in one dish was covered with glycerin to a depth of about 4 mm. The light from an arc lamp was then reflected from

the two dishes directly to a photographic plate, exposed behind a "focal-plane" shutter. The mercury waves can scarcely be seen in the image (fig. 15) obtained from the glycerin-covered surface, while the other image (fig. 16) speaks for itself.

It seems quite possible that the glycerin cover may prove useful in making artificial horizons. Inasmuch as my twenty-inch mirror now runs with so little error and with such insignificant ripples that the maximum deviations from perfect focus amount only to a millimeter or less

FIG. 16.

(and this with a 5-meter focal length), I felt very confident that the glycerin cover would make the image practically perfect.

Castor oil was used instead of glycerin, as the latter fluid absorbs moisture from the air and develops striæ. Experiments showed, however, that the oil served only the purpose of entrapping insects, which previously gave much trouble by bumping into the mercury surface. The long period or tidal waves are not destroyed by the viscous fluid, and these are the

only ones which remain. I have mounted a 15-inch flat (silvered glass) over the telescope pit, and can now reach almost any part of the sky. In the case of the moon, I have had momentary glimpses of the smallest craters which are visible in my 5-inch refractor.

The image becomes very sharp once in every two revolutions, *i.e.*, once in every ten seconds. The cause of this periodicity has not yet been determined. It is perhaps due to a combination of the period of rotation with the natural period of vibration of the concave mercury surface. Further experiments will probably throw more light on the slight residual troubles, and it may be possible to remedy them.

I feel compelled to make an apology to your Society for introducing a description of an instrument of very doubtful value into a Thomas Young oration. It is possible that these experiments may be of value in suggesting ideas to others, and something may perhaps eventually develop out of the thing along some line quite other than that originally planned. The man who invented the "key that would wind any watch" lived in poverty, as his invention was brought out at the time when stem-winders were becoming popular. Then someone made the "automatic pencil" which operated on the same principle, and the watch-key man sued him for infringement, and lived happily ever afterwards on the royalties which the pencil man was obliged to pay him.

SURFACE BRIGHTNESS AND A NEW INSTRUMENT FOR ITS MEASUREMENT.

By J. S. Dow and V. H. Mackinney.

Paper read October 13th, 1910.

It is very interesting to trace the development of the measurement of light, and to observe how, as new methods of illumination were introduced, new views came to be entertained on the subject.

Originally, one might almost say, the whole subject of photometry was regarded merely as an interesting scientific playground.

But eventually, as artificial illumination developed, the practical importance of the subject came to be more fully recognised, and many of the early anomalies were removed. The development of gas lighting early in the last century had much to do with this change of view, and we find that a considerable amount of early work in practical photometry was concerned with the measurement of the illuminating power of gas. It was recognised as important that the gas supplied should have a specified illuminating power, and explicit tests were laid down to secure compliance with a certain standard. Subsequently, as new types of burners appeared, the desirability of measuring the illuminating power of the source of light burning this fuel was also appreciated. It was soon seen that some sources made better use of the gas supplied, and were able to produce light more economically than others. The introduction of so many different lamps, both gas and electric, served to bring home this idea more closely yet, and the recent standard specification issued for

the testing of electric incandescent lamps may serve as an indication that the importance of testing sources of light is now perfectly well established.

But as early as 1883 Sir Wm. Preece pointed out that, although measurements of the illuminating power of sources of light are very desirable, they do not, strictly speaking, tell us exactly what we desire to know. What we are really anxious to learn, he pointed out, is not only the light of the lamp given by the lamp used but *the actual intensity of illumination* "on the surface of the book we are reading, or the paper on which we are writing, or of the walls on which we hang our pictures," etc. He, therefore, proceeded to describe an instrument for the measurement of illumination. Since that date a number of instruments of this kind, variously known as "illumination-photometers," "illuminometers," etc., have been designed, and have proved a valuable asset in studying practical problems in illumination *

Yet it might legitimately be argued that even a measurement of illumination does not exactly describe practical conditions. The connection between intensity of illumination, and the candle-power of the source illuminating is represented by the well known formula:—I, the intensity of illumination = C. cos α/d^2 (where C is the candle-power or intensity of the source, d its distance away, and α the angle of incidence of the rays striking the surface illuminated).

The intensity of illumination, therefore, is a quantity which is independent of the nature of the surface on which the light strikes. By introducing into the formula the co-efficient of reflection ρ, of the material illuminated, we obtain the "surface-brightness," which is C cos

* For a description of instruments of this kind, see Millar, Trans. Illum. Eng. Soc., U.S.A., 1907.

$a.\rho/d^2$—*i.e.*, the actual brightness which the surface appears to the eye to possess.

It is rather curious that this conception of "surface-brightness" has not been much utilised in this country, although in Germany the term "Flächenhelligkeit" is widely understood. However, we do meet with the quality in connection with very bright surfaces, the term used being in this case "Intrinsic brilliancy"—regarding which more will be said later. Strictly speaking, it is almost invariably the surface-brightness of an object which most closely represents practical conditions. For this term describes what is actually seen by the eye. It is the value of the surface-brightness of the page of a book, for example, which enables us to read, and it is mainly the contrast between the surface-brightness of adjacent objects which enables us to distinguish them one from the other. The authors are, therefore, inclined to suggest that the measurement of this quantity would often be of considerable service in connection with practical lighting problems. Naturally, this does not mean that the measurement of illumination is rendered superfluous, nor that it is not still as essential as ever to measure the candle-power of the lamps we use and the illuminating or calorific value of the gas we consume. But the measurement of surface-brightness would seem to be a logical method of studying practical conditions, and an important link in the chain of processes connected with the study of illumination, all of which have their sphere of usefulness.

Strictly speaking the above two terms are used to denote the same quantity. There are, however, two methods of expressing surface-brightness which Professor L. Weber has termed "primary" and "secondary." We may either consider the brightness of a surface from the standpoint of the light falling upon it, in which case it is convenient to express it in terms of "foot-candles." For example, the brightness of a wall paper, a page of a book, or a pavement

in the street may be so described. Or, in dealing with surfaces of great brilliancy, which emit sufficient light to constitute effective sources of light, we may express the brightness in terms of "candle-power per unit of area." In the latter case the term "intrinsic brilliancy" seems to be very generally used, this method being almost exclusively applied at present to describe various sources of light, shaded lamps, etc. This term has become very widely known recently owing to the stress now laid on the importance of avoiding the glare of modern illuminants and the recognition that the intrinsic brilliancy is in many cases inconveniently high. Stockhausen and other observers have published tables of the intrinsic brilliancies of a large number of lamps.* In passing it may be suggested that it might be well for us at this early stage in illuminating engineering nomenclature to draw a distinction between intrinsic brilliancy and surface-brightness, and to apply the former term to surfaces which actually generate light, and the latter term to illuminated surfaces, which only become visible by reflected light. It is also convenient, on account of the size of the numbers involved, to use "candle-power per square inch" in the former case, and "foot-candles" in the latter.

The surface-brightness is merely the intensity of illumination, I, multiplied by the co-efficient of reflection ρ. The connection between surface-brightness, ρ I, and intrinsic brilliancy, B, as thus defined is a very simple one—

$$\pi B = \rho I. \dagger$$

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* Illum. Eng., London, Vol. III., February, 1910, p. 113.

†The following proof of the relation between surface-brightness, as defined above, and intrinsic brilliancy, is based on a theorem in Liebenthal's "Praktische Photometrie" (p. 34).

Let σ be a unit element of surface which has an intrinsic brilliancy B and a surface-

It may be noted that both quantities must be expressed in the same units, so that if foot-candles and C.P. per square inch are used the numerical relation is:—

Surface-brightness, $\rho I = \pi \times 144 \times B$.

$= 450\ B$ (approx.).

It will be seen that the surface-brightness of such a surface as an incandescent filament may amount to many thousand foot-candles, and is, therefore, preferably expressed in C.P. per square inch.

In saying that a certain object possesses a surface-brightness of so many foot-candles, or lux, we merely mean that a truly white surface would have to receive this intensity of illumination in order to have an equivalent brightness. The ratio of the surface-brightness to the illu-

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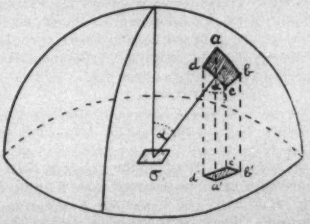
brightness $I\rho$. The total amount of light emitted by this unit surface is then, also, $I\rho$, and its candle-power, in a direction perpendicular to the surface, is B.

Imagine this surface to be surrounded by a hemisphere of any radius, r, as shown in the figure, and consider the flux of light falling upon any small element of surface thereon, $a\ b\ c\ d$, of area, s, which the rays of light strike at an angle of incidence α. The flux of light



distributed over this area, $a\ b\ c\ d$, is then $B.s.\cos\alpha/r^2$, and the expression for the total

mination is thus a measure of the reflecting power of the material.

It is always possible to estimate indirectly the intensity of illumination from surface-brightness if a knowledge of the reflecting power of the material is known, or *vice versa*, though there do not seem to have been many instruments devised with the object of measuring surface-brightness directly. An apparatus of this kind was, however, described by Dr. C. H. Williams, in the United States, a few years ago, and a Russian, Petruschewsky, appears to have designed and described a somewhat more elaborate form of apparatus for studying the lighting of children's desks in school, while Bouguer also contrived a primitive instrument for measuring the intrinsic brilliancy of stars, etc., at an earlier date.

In the case of such surfaces as incandescent filaments, which are both exceedingly brilliant and of small dimensions, it seems questionable whether any direct attempt to compare their brightness with that of some other illuminated surface is worth the trouble involved, and it would probably be best in such cases to determine the intrinsic brilliancy indirectly by

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amount of light distributed over the whole hemisphere is $B \Sigma s. \cos \alpha / r^2$,

But we notice that the angle α is the same as the angle of inclination of the surface, $a\ b\ c\ d$, to the plane in which the surface σ is situated; therefore, $s. \cos \alpha$ is equal to the projection of $a\ b\ c\ d$ on this plane, *i.e.*, $a'b'c'd'$. Similarly $\Sigma s. \cos \alpha$ is equal to the sum of all such projections, *i.e.*, to the area of the great circle of the sphere πr^2.

The total flux of light emitted by σ (assuming that it obeys the cosine law accurately), is, therefore, $\frac{B. \pi r^2}{r^2} = \pi B$. We have also seen above that the total flux of light is, by definition, $I \rho$. Whence the relation $\pi B = I \rho$.

measuring the candle-power and superficial area, as recently advocated by Krüss.* Lux,† however, has devised a special form of laboratory apparatus for determining the brightness of small surfaces of this kind with great exactitude.

But in the case of many of the surfaces we meet with in daily life, illuminated papers, walls, placards, etc., a direct measurement of surface-brightness would often be convenient, and it occurred to the authors that a description of the "Lumeter" instrument recently devised by them and previously described, in crude form, before the Illuminating Engineering Society (London),‡ might be of interest.

Before proceeding to the description, however, it may be well to refer to one problem which plays an important part in its design.

It is common knowledge that the brightness of any object does not very materially alter as we increase our distance away, except, of course, owing to absorption from the intervening atmosphere, in very foggy weather, and when the distance away is very great indeed. Theoretically one would expect this to be the case since the amount of light entering the eye, and the size of the retinal image, both decrease according to the square of the distance; the objective surface-brightness of the image should, therefore, be constant. Nevertheless, its *apparent* brightness may vary owing to the complexities of the retina. This is particularly the case for very weak lights, the centre of the retina being known to be very insensitive to light at low illuminations.

In the instrument about to be described, however, retinal peculiarities do not interfere in

* Illum. Eng., London, Vol. III., February, 1910, p. 129.

† Illum. Eng., London, Vol. III., March, 1910, p. 184.

‡ Illum. Eng., London, Vol. III., June, 1910, p. 373.

the way described since the portion of the retina used is always the same, and we look at the surface we wish to study through an aperture in an illuminated screen maintained at a constant distance from the eye. The other factor, absorption of the atmosphere, is, of course, still potent. But in practice one finds that at moderate distances, and in ordinary weather, there seems to be very little effect. Otherwise accurate photometry would be difficult, if not impossible. It may also be pointed

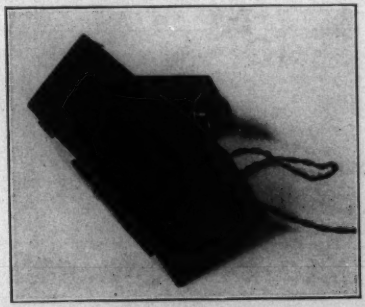

FIG. 1.—GENERAL VIEW OF "LUMETER" SURFACE-BRIGHTNESS PHOTOMETER.

out that an instrument of this kind presents a very convenient means of studying this question of atmospheric absorption, and the authors hope to be able to make some experiments on this point when the weather conditions prove favourable. It might also be suggested that, even though the brightness of an object were found to diminish with distance owing to atmospheric absorption, when studied with this instrument, the value so obtained is a genuine one for our purpose, because it represents *what is actually seen by the eye.*

The general appearance of the instrument will be understood from fig. 1. It enables an observer to measure the surface-brightness of any surface by comparing it direct with a standard illuminated white one. The manner in which this is done will be understood from fig. 2. The observer looks direct at the illuminated screen, which is seen here, taken out of the instrument, and resting against its side, and sees, through an aperture therein, the illuminated object to be studied—for example, a picture, or some goods in a shop window. The sur-

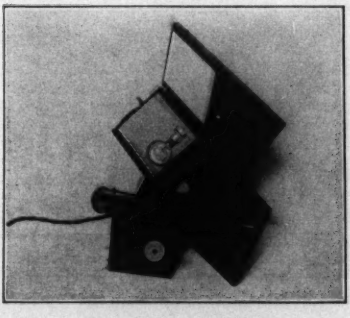

FIG. 2.—DETAILS OF "LUMETER" INSTRUMENT.

face is illuminated by an opal glass screen, which is also clearly visible in the figure, and has a sector shape. Behind this is a small metallic filament glow-lamp at a sufficient distance to render the screen of uniform brilliancy.

The method of altering the illumination is somewhat unusual. The screen S (see fig. 3) is covered by an opaque diaphragm D, which only allows a sector to be exposed. In front of this evenly illuminated sector two correspond-

ing opaque screens, attached to levers A and B, can be made to pass. As the screen A is moved in front of the sector its area, and, therefore, its intensity as a light-source will be uniformly diminished from, say, 1 to 0.1. The remaining intensity, 0.1, is derived from the part of the surface still visible through the ring R. The area of R is arranged to be exactly 1-10th of the total area of the bright sector. The lever attached to A, therefore, travels on an equally divided circular scale with 9 divisions from 1 to 0.1.

Having drawn across the lever A, we can gradually reduce the illumination still further by also bringing over the lever B. This exactly

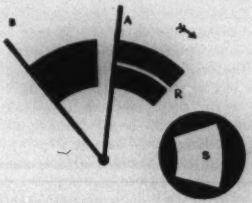

FIG. 3.—DIAGRAMMATIC VIEW OF SECTOR DEVICE.

resembles A, with the exception that there is no ring cut out. In doing so we gradually cut off the illuminated portion of the opal plate still left exposed and reduce the illumination uniformly from 0.1 to 0. The lever B travels on a second scale immediately above that of A, graduated from 0.1 to zero.

In actually using the instrument one would adjust the position of the glow lamp illuminating the opal plate until the brightness of the white surface S (as compared with another similar white surface illuminated by a standard lamp at a specified distance) is, say, exactly

one foot-candle. One can then fix the lamp (which slides to and fro) in this position and regard the scales as registering in foot-candles.

So far a range of illumination from under 1/100th to one candle-foot can be measured, and the method of arranging the scales adopted is specially convenient for the study of very weak illuminations such as are met with occasionally in street lighting. (It must also be remembered that the surface-brightness of many objects is very much less even than the low illumination with which they are furnished.) Possibly the same arrangement will also be well adapted to the studying of phosphorescent surfaces, where very transient effects have often to be observed.

As an illustration of the low intensities which may be measured in this way it may be mentioned that on several occasions we were able to estimate the brightness of the sky at night-time (which is by no means destitute of light). In one case a surface-brightness of 0.008 foot-candles was noted, but the sky immednately over the flame arc lamps at Charing Cross was considerably brighter—over 0.015 foot-candles.

In order to extend the range upwards we propose introducing two dark glasses, each giving a multiplying power of 1/10th, and together a multiplying power of 1/100, in the path of the rays of light studied. In this way an illumination up to 100 foot-candles could be measured. Even this, however, is not high enough to enable us to obtain direct readings of the intrinsic brilliancy of lamp-shades, etc., which frequently exceeds 5 or 10 C.P. per square inch, and we are still working on other means of reducing the incident light in a satisfactory manner without affecting its quality. It is possible that dark glasses can be made to serve this purpose, but as far as our examinations of such glasses has yet gone, there seems a difficulty in making use of the principle of absorption to obtain a reduction much greater than that specified

above. Many glasses which in thin specimens appear to be quite neutral, prove to be almost ruby-red when used in considerable thickness. However, a reduction to 1/100th in the incident light, with quite sufficient accuracy for practical purposes, should present no serious difficulty.

In passing there is one other feature in the instrument that may be mentioned as interesting to those concerned with the manufacture of photometric screens. In this case the screen has been made by depositing a special matt white precipitate on thin glass and then scraping away a central disc. In this way a very fine line of division between the photometric surfaces can be secured. The method was suggested by an attempt to modify the ingenious mirror-screen recently described by Mr. L. Wild.* There were certain initial difficulties to overcome, but experience has shown that there is no difficulty in securing a satisfactory matt and permanent surface when the precipitate is suitably prepared; this device, like many others utilised in the instrument, is due mainly to the ingenuity of Messrs. Conrad and William Beck. The screen is subsequently covered by a second thin glass plate, which is bound in position. The actual white surface is thus preserved from air and moisture; the glass cover enables the screen to be handled with safety and easily cleaned. The position of the screen is such that no direct reflection from this surface into the eyepiece is possible.

The uniform scale of candle-power secured by the moving shutter device is naturally a considerable advantage. It enables the scale to be very conveniently and easily divided on principle and not empirically. As might have been anticipated, however, no little care is necessary in proportioning the various parts and in overcoming the various possible errors. For

* Illum. Eng., London, Vol. III., 1910.

the success of the method it is essential that the ring cut out should be exactly 1/10th of the illuminated area, and that the distribution of illumination over the latter should be absolutely uniform. In addition the opal plate must be highly diffusive; in the early type of instrument it was found that considerable errors might arise through the fact that a proportion of the light was transmitted direct through the opal plate. This defect was largely got over by using a fairly thick plate and having both the inner and outer surface well ground. The diffusion and uniformity of illumination was also considerably assisted by making the interior of the chamber in which the lamp is placed a matt white, on the principle of the Ulbricht Globe.

Some idea of the gradual elimination of errors in the scale (as determined experimentally by utilising the inverse square law) will be gathered from fig. 4.

As far as practical measurements are concerned an accuracy of the scale to within 5 per cent. would be considered ample, perhaps even excessive. While this instrument will probably be used mainly for measurement of illumination in practice where extreme accuracy is not very essential, it was still thought desirable to aim at a precision of the scale near the above limit, in order that it might also be of service for many varieties of laboratory work.

The first three curves, 1, 2, and 3, are typical of a large number obtained with the original model previously described before the Illuminating Engineering Society.* The usual course of these early distributions of error is strikingly shown in Nos. 1 and 3. The curve in each case rises to a maximum, falls, and finally rises sharply once more as we approach 0.1 on the scale. We were led to suspect that the peculiar shape was partially due to the fact that the

* loc. cit.

opal plate (which in the very first model was not sufficiently ground) let through some light by direct transmission. The result of this would be that some light from the lateral portions of the sector would pass out obliquely without being diffused, and would not strike the screen, as suggested in fig. 5. By grinding both surfaces of the opal plate matt, and removing the lamp to a greater distance, the error was distinctly reduced, and curve No. 3 was the result.

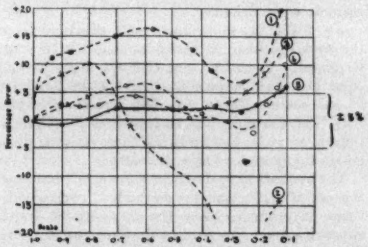

FIG. 4.—CURVES SHOWING GRADUAL ELIMINATION OF ERROR OF SCALE. NOS. 1, 2, AND 3 ARE AMONG THE VERY EARLIEST TESTS OF THE FIRST MODEL; 4 SHOWS THE IMPROVEMENT SECURED BY VARIOUS MODIFICATIONS; 5 (SHOWN THICK) IS THE FIRST TEST OF THE SCALE OF THE LATER MODEL.

At this stage we were surprised to find that the shape of the error-curve depended on the nature of the lamp used, a new form of filament in one case resulting in No. 2. This was really a natural result of the error described above. It appeared that the whole curve was bodily shifted to and fro according to the position of the centre of radiation of the filament. But this defect was remedied in a subsequent design, and other alterations, including the use of a

whitened lamp chamber, enabled us to obtain No. 4, which was again an improvement.

As regards the curious rise in the error-curve as it approached 0.1, this is due to a secondary source of error, namely, the want of accuracy in apportioning the area of the ring cut out from the shutter (R in fig. 3). This error is, naturally, masked when the shutter is half across owing to the bright area exposed being relatively large in comparison with that of the ring, but it becomes very marked as the end of the scale is approached.

No. 5 (fig. 4) shows the result obtained from the first specimen of the final model of the instrument. It will be seen that the error proved to be less than 3 per cent. over the greater part of the scale. There was still evidence of the "tilt" at the end, due to incorrect proportioning of the ring, but we think that we have since got to the bottom of this difficulty.

The authors have dwelt in some detail on these sources of error, and the methods of overcoming them, in order to show the necessity of strict supervision on the part of those responsible for instruments of this kind. There are, of course, many other small points which demand attention, but experience has shown that they can all be overcome by sufficient care. The mode adopted of varying the illumination of the photometer screen in this instrument seems to be capable of yielding very accurate results, provided the parts are scientifically designed and carefully made. On the other hand any lack of precision in these respects is detrimental, and the authors have, therefore, determined to check every instrument personally before it is sent out for use in order to ensure that everything is satisfactory.

Of course it must be understood that these are difficulties in design and construction merely, and not in use. Once the instrument is properly designed, and correctly constructed, it should give anyone using it no trouble; it is

specially designed with a view to simplicity and convenience, and, as the various parts are readily accessible, and are not easily deranged, should also prove reliable with reasonable care.

On theoretical grounds one would expect that the apparent brightness of any surface, as observed with the instrument, would remain constant as the distance of the observer is increased, and this is borne out by practice. For example, during one of a series of tests on this point, a uniformly illuminated surface was observed at distances of from one to twenty feet, without any appreciable alteration in brightness being detected; the distance could probably have been increased still further without error, but that the image of the surface viewed began to fall inside the aperture in the photometric screen. Provided, therefore, that the aperture of the screen is completely filled by the surface studied, the distance of the observer seems to be immaterial.

That this would also prove to be the case, when a lens was used in the eyepiece, seemed less certain, and with the arrangement employed in the original model, there did seem to be signs of a slight change as the eye receded. With the lens-system finally adopted, however, very constant results at different distances are obtained; and if the lens is removed from the telescope and the naked eye is used the reading of the instrument does not seem to be appreciably altered.

In general the eye should focus on the edge of the aperture. But even if it inadvertently does not do so, with the result that this edge appears "fuzzy" for the time being, the photometric balance of the field of view does not seem to be perceptibly affected.

In testing these points there is one matter which should be carefully borne in mind, namely, that the surface studied should be really evenly illuminated. Naturally, as one moves away from the surface, a larger area must be

viewed in order to cover the aperture in the screen. Therefore, if the more remote portions of the surface are less brightly illuminated than the centre, its apparent brightness, as a whole, appears to decrease as the eye recedes. It appears less bright because it really is so, and in this respect the instrument only records the same impression as that received by the eye in actual practice.

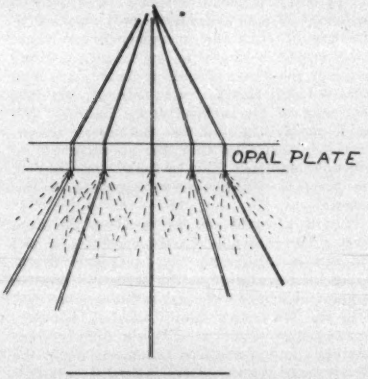

FIG. 5.—SHOWING MIXTURE OF DIFFUSED AND DIRECTLY TRANSMITTED LIGHT.

The use of the instrument to measure surface-brightness has already been explained. If one wishes to ascertain the surface-brightness of any object one merely looks at it through the illuminated photometric screen and adjusts the illumination of the latter so as to secure balance. As explained above, the distance away of the object is immaterial. This constitutes a decided

advantage, for one can study inaccessible objects, such as distant placards, walls, bookshelves, or even objects in a shop-window behind glass. The separation of the two photometric surfaces in this way seems to be a very logical method of enabling one to measure brightness in any plane, in a recess, and, in short, in any spot in which it is difficult to place the actual instrument.

It is also an easy matter to measure intensity of illumination. In this case all one has to do is to place a standard white surface, which has

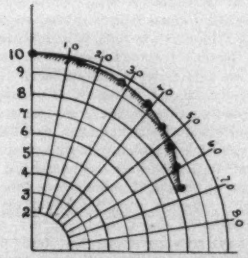

FIG. 6.—SHOWING VARIATION IN BRIGHTNESS OF WHITE CARD, WITH GLAZE IMPERFECTLY REMOVED, WHEN VIEWED AT DIFFERENT ANGLES.

previously been employed to calibrate the instrument, at the spot where we desire to know the illumination and to examine its brightness in the usual way. It may be observed that although it is desirable to use as white a surface as possible for this purpose, a perfectly white tint is not really necessary, as far as tests of illumination are concerned. The main requirement is that the same surface should be first used in calibrating the instrument from a standard source, and then employed in subse-

quent tests in which this calibration is assumed.

It is also clearly essential that the standard surface should be as matt as possible, in order that light striking it from any angle may be diffusely reflected, and its apparent brightness may not vary when observed from different directions. This is, of course, a requirement in the surface used in any illumination photometer. As far as practical requirements are concerned, however, there is very little to fear in this respect if reasonable care is taken. Fig. 6, for example, shows the difference in reading resulting from observing a piece of imperfectly matt white cardboard, normally illuminated, at various angles. In studying illumination with this instrument the authors have hitherto followed the convention of looking vertically at the surface. All that seems really needed, however, is to observe the illuminated surface at about the same angle as that used when the instrument was calibrated. However, with a reasonably matt surface, a wide variation in angle may be permitted without any error of practical consequence creeping in. In order to secure very consistent results indeed plaster of paris and pressed magnesia screens have been recommended, but for practical purposes good white cardboard or celluloid, from which any glaze has been removed, answers. It is often more convenient to place in position in inconvenient corners, is cheap, and is readily replaced when soiled. The size of the surface employed naturally depends upon the distance away from which it is likely to be observed. For near work a piece about 3 or 4 inches square is often ample, and the ability to use such a small surface enables one to penetrate into crannies in which it would otherwise be very difficult to obtain a measurement.

One case in which white card or paper is convenient is in getting a rough idea of the reflecting power of wall papers, curtains, etc. All that is necessary is to pin up the card on the material

in question and take first a reading of the illumination on the white card, and then a reading of the brightness of the adjacent material.

By a very simple addition the instrument can also be utilised to measure the candle-power of lamps in a room or street. A small tube terminating in an opal glass plate, frosted on both sides, is inserted in front of the photometric screen and presented towards the lamp studied. The observer sees this plate through the aperture and determines its intensity of illumination. Then, knowing the distance away of the source, he can readily calculate its intensity. It is, of course, necessary to determine the constant of the instrument, used in this way, by observation of a standard lamp of known candle-power. Once this constant is determined, however, we have at hand a convenient way of forming an approximate idea of the light given out by a lamp. We merely present the photometer towards it, taking care that the rays strike the opal surface vertically, take a reading, and measure the distance away of the lamp. One can thus measure the candle-power of a source in position, and without the necessity for an unduly elaborate or cumbrous photometrical bench. Of course it need not be said that certain precautions are necessary, as in all measurements of candle-power. It is necessary to ascertain that no stray light reaches the opal surface from other sources than the one studied, or by reflection from walls, etc.; or, at least, that no material error is introduced in this way. As the instrument can usually be brought fairly close to the source studied, however, it is, as a rule, not difficult to avoid serious error from this cause, even in a room with fairly light walls.

This instrument, like all others intended to measure illumination under practical conditions, requires calibration occasionally. Natur-

ally the voltage of the accumulator eventually falls, and the small glow lamp used also suffers a diminution in candle-power. It is therefore essential to check the instrument now and then and to regard it, not as a permanent standard, but as a temporary custodian of a standard of illumination and surface-brightness which can be relied upon with confidence for a sufficiently long period, but requires calibration now and then.

Before taking the instrument out for a prolonged test, therefore, it is desirable to check it in the following manner: One sets up the standard white surface which is to be used in the experiments and arranges a standard source of light at such a distance from it that the illumination is exactly one foot-candle. One then looks at this approximately vertically through the aperture in the screen of the instrument, and slides the small glow-lamp backward and forward until an exact photometric balance is obtained with the pointer at 1.0 foot-candles. (A somewhat better arrangement is to calibrate at about 0.7 or 0.5 foot-candles, as in this case one can manipulate the pointer on either side of its correct position.) After calibration, the lamp is clamped in position and the instrument need not be adjusted any further until prudence suggests that the apparatus should be checked again. As far as the measurement of illumination is concerned it is clearly not very material that the standard surface should be very accurately white, since any error in this respect tends to cancel out. As, however, readings of illumination may be compared with readings of surface-brightness of various materials, a good matt white screen is desirable.

As regards the method of obtaining the illumination on the standard screen, it is naturally preferable to have the resources of a laboratory at one's disposal and to employ a carefully standardised electric lamp or flame mounted on a photometric bench; indeed, this is essential

when the order of precision aimed at in accurate laboratory work is desired.

But in most practical cases it is admitted that no such accuracy is demanded, and a result that can be relied upon, even within 10 per cent., would often be considered satisfactory. When examining the illumination on the desks of school-children, for example, an inspector would not be exceedingly anxious to know whether the illumination was exactly 1.8 or 2.0 foot-candles; but he *does* want some means of ascertaining and proving whether it is 1/10th or 10. It may be suggested that for many purposes a sufficiently practical check could be secured by merely examining the illumination at one's study table, or in some situation, where measurements have constantly been made and the intensity of the light has been found to remain fairly constant. For example, one of the writers has been in the habit of taking readings on the table in his library at home, which is lighted by three tungsten lamps, in Holophane pines. Although no particular care has been taken in standardising the instrument over this period and the accumulator was frequently changed, the reading has never been lower than 1.75 foot-candles, or higher than 2.00, and this variation might quite easily have been caused by the fluctuations in the supply voltage.

There is, however, a need for some approximate and portable standard of light which could be carried about by anyone on tour with an instrument of this kind, and the authors have been considering the possibility of contriving a serviceable lamp. Meantime, it may be suggested that although the standard candle is rightly regarded as unsatisfactory in the light of a national standard, from the purely scientific and laboratory standpoint, it might yet be found serviceable in the connection suggested above. The writers have made some experiments on such candles with, on the whole, encouraging results. Provided a few simple

precautions are taken, the value of these candles was found, when burning normally, to be in close agreement with the international unit. Some little care is certainly necessary. For example, the candle must not gutter, and should be allowed to burn steadily for about 10 minutes until a cup of molten wax is formed before a reading is taken. The wick should also not be too straight, nor should it sag over into an almost horizontal position, as the light is reduced and increased respectively beyond the normal by such changes. But after a little practice an observer would probably have no difficulty in assuring himself when the correct conditions in this respect were attained. There is, of course, the effect of atmospheric conditions on the light to be considered. On the whole, however, the standard candle seems to afford a serviceable means of checking the instrument when more elaborate methods are not available, and when the order of accuracy demanded by practical work is borne in mind. It need hardly be said that certified standard candles should be obtained, and that *any* candle will not do.

What has been said above relates to absolute determinations of illumination. But a very large number of practical uses of the instrument involve *relative* readings only. One may, for example, desire to show that the illumination on a table is increased by using a particular reflector. Or one may be only concerned with the fluctuations in the illumination, or with its distribution. All that is demanded in such cases is that the instrument should not alter while the test is being made.

It may be added that the instrument is provided with a push-switch, so that the current is taken from the accumulator only for very short intervals of time, and the cell should only run down very gradually. In practice it seems to be preferable to discharge an accumulator for a few minutes after it has just been charged in order to run off the intial ab-

normally high voltage. For example, a recent test showed that when a newly charged accumulator was substituted for that previously used the reading of the instrument was about 5 per cent. over that obtained when it was last checked. As an illustration of the rate at which the voltage falls it may be mentioned that on one occasion the instrument was used at intervals during a fortnight, without the cell being charged or the reading checked, and when the instrument came to be readjusted the fall in the candle-power proved to be only 9 per cent. It may be added, moreover, that in this case the accumulator used was one of only half the capacity which we propose to employ.

The great benefit of a measurement of illumination or surface brightness is that it provides a definite record. In many cases one may feel confident that the illumination is too low, but unless the order of illumination demanded by the circumstances is clearly understood, and one has an instrument by which the illumination provided can be actually measured, it is often difficult to substantiate one's opinion. An inspector of schools may consider the lighting of a school room faulty; a factory inspector may think that more, or better distributed, light is needed in a workshop; a passenger on a railway may feel convinced that he has not enough light to read by. But how is it possible to put on record the conditions in these cases without some method of measurement?

The same difficulty is met with when a change in an installation has been made, and it is impossible to reproduce the old conditions of lighting. If, under these circumstances, a client is unwilling to admit that he receives more illumination than he previously did, how is one to convince him without measurement? It may be that the illumination under the old circumstances was incontestibly insufficient, or badly distributed, but the client may retain but a misty memory of what the conditions were

like before the change was made. He may admit that the lighting is now good, but he may be inclined to imagine that it is no better than it used to be. He may even suspect that the saving in his lighting bill has been accomplished only by cutting down the light! In such circumstances a measurement of the actual illumination before and after the change might be very serviceable.

The same thing might occur to a salesman who attempted to demonstrate the special benefit of some form of globe, reflector, or fixture. It might be admitted that an improvement had been made, but the question would remain, how much?

In many cases, too, one finds that people have methods of their own of judging whether or not a room is well-lighted. It is only after interrogation that one discovers that their conception is based, not on the appearance of the room and the illuminated objects within it, but on the intrinsic brilliancy of the sources employed. A demonstration of testing illumination is, therefore, a valuable educational process and an excellent method of drawing their attention to what should really be aimed at.

As an illustration of how one's judgment of the brightness of surfaces may be deceived, we may mention an experience, on a recent September afternoon, at about 5.30 p.m., when dusk was coming on. We amused ourselves by measuring the brightness of various surrounding objects, and, although familiar with the high order of brightness prevailing by daylight, were surprised to find that the surface-brightness of the dark tree trunks was in some cases, actually as high as 3 to 4 foot-candles. Subsequently we went into a teashop at Charing Cross, and the effect was that of coming into a brightly-lighted room, from the dim twilight outside. Yet will it be believed that the surface-brightness of the apparently strongly illuminated white marble tea-table was under 2 foot-candles—actually

lower than the dark tree-trunks of the trees outside? The explanation of this curious underestimate of the brightness of surfaces illuminated by daylight seems to lie in the fact that there is always a wide expanse of sky, with a surface-brightness of many hundreds (and occasionally thousands) of foot-candles, stretched overhead, so that all moderately bright objects appear dark in comparison.

The chief advantage of surface-brightness measurement is that it enables one to go right to the root of the matter. With the instrument described one can determine not only the illumination provided by the lighting engineer but the resulting actual brightness of the objects illuminated. Another advantage is that in studying distribution of illumination, surface-brightness is often a much more convenient thing to measure than illumination. For example one can determine the distribution of light over a large placard or the face of a building by merely pointing the instrument in the desired direction, and in this way can reach parts where it would be impossible to place the actual instrument.

In the same way a measurement in a horizontal plane alone might fail to give an adequate idea of what the desirable illumination on a piece of machinery or a tool in the workshop should be. In such a case it might be suggested that the conditions would be best studied by mounting the small white test plate at the exact point where a good illumination was needed, and one might thus come to form an idea how light ought to be distributed on such tools, and what order of illumination is needed for different classes of work.

As an extreme illustration of measurements at a distance it may be mentioned that we attempted to measure the surface-brightness of the dial of the clock in the Westminster Tower, which, viewed from the lower end of Whitehall, will just cover the aperture in the instrument.

Owing to the fact that the surface is interlaced with opaque lattice work, it is not easy to get an exact reading from the pavement below, but the apparent illumination seemed to be between 1 and 2 foot-candles.

At close quarters the method is also advantageous, owing to the fact that such a small portion of the surface studied is needed in order to obtain a reading. Thus the apparatus is very convenient for studying the distribution of light beneath table lamps (which, by the way, is often curiously uneven!). Another feature of measurements of surface-brightness is that one can often obtain useful information by studying the brightness of inaccessible objects inside glass cases, *e.g.*, one can study the contents of shop-windows without it being necessary to take the instrument inside among the goods. It seems reasonable to suggest, too, that an excellent method of judging whether a show-window is well-lighted or no, is to study the appearance of the lighted goods themselves.

In passing it may be remarked that a quite remarkable diversity seems to exist in the brightness of objects in show windows, even of the same class, values varying from 1/5 to 50 foot-candles being observed.

In conclusion, reference may be made to a few experiences of the use of the instrument in various interiors, etc.

Dwelling Rooms.—The ease with which one can make measurements in any plane and at a distance should here be very beneficial. From a decorative standpoint one of the first things that concerns us is the surface-brightness of the wall papers used. The following table shows the application of the instrument to determine both the illumination and the surface-brightness in various rooms in a house. As explained previously, all that is necessary in such measurements is to pin up the white surface on the wall and to measure first the brightness of the white surface and then that of the adjacent wall

paper. An incidental advantage of the method is that when the eye is fixed on the aperture, the surface behind is out of focus; consequently one is unconscious of the pattern of the paper studied and automatically obtains a mean result, when the parts of the pattern reflect light to a differing extent.

There seems to be a lack of data regarding the desirable surface-brightness of walls, although general figures have sometimes been quoted for the illumination. The four rooms studied are probably fairly typical, being drawing room, hall, library, and dining room respectively. Judging from these experiences one would be inclined to suggest (with reservations) that a "cheerful" room should have walls with a surface-brightness, by artificial light, of not less than 0.3 foot-candles. No doubt in many rooms with very light walls much higher figures than this would be obtained, however. In the rooms with the red and green papers (respectively hall and library), the lower order of brightness corresponds with a more subdued effect in the surroundings, and this seems well

TABLE I.

Reflective Power of Wallpapers.

(From actual results in rooms lighted by Tungsten lamps.)

Nature of Paper	Surface-Brightness. (Foot-candles)	Illumination (Foot-candles)	Reflecting Power. (Approx.)
Light Blue	0.3	0.72	40%
Deep Red	0.15	0.6	25%
Deep Green	0.15	1.0	15%
Very Deep Blue	0.015	0.35	4.5%

adapted to the purpose of the interiors. The wall paper in the dining room, it must be confessed, is very deep indeed. In a dining room one should probably purposely aim at rather a massive impression and subdued surroundings in order to make the festive table stand out by

contrast; but it would appear that in the room in question this effect has been overdone, and the result is somewhat funereal.

In estimating the amount of light reflected from deeply coloured materials one cannot, of course, expect the same order of accuracy as when making photometrical measurements with lights of the same tint. But under practical conditions a very fair order of accuracy may be expected, and it might be supposed that this simple method of studying the tints and absorbing powers of various surfaces would commend itself to architects and decorators.

One can, of course, also easily ascertain the intensity of illumination on tables, bookshelves, etc., by attaching thereto the little white surface on the spot where a measurement is desired. In the library mentioned above the vertical illumination on the bookshelves is rather over one foot-candle, and the horizontal illumination on the table in the neighbourhood of 1.8 foot-candles.

The same remarks might be made regarding quality of paper employed. One is too often inclined to suppose that if an illumination of one foot-candle is provided one has done all that can be expected to render reading comfortable. But it must be borne in mind that the paper on which the characters are printed may reflect but a fraction of the light impinging upon it. When one is reading a newspaper in a lighted train one frequently suffers through this fact as well as through deficient illumination. The following figures were recently obtained for three well known newspapers:—

TABLE II.

	Reflecting Power.	
White Cardboard	... 100	
'Westminster Gazette'	... 90	(very approx.)
'Globe'	... 80	" "
'Sporting Times'	... 53	" "

And it may be added that the cardboard in question was not as white as might have been selected, so that the results are, if anything, high. However, many of the papers in use, such as blue tramway tickets, some theatre programmes, etc., reflect very much less light than the above. This is one more instance of the manner in which the lighting engineer finds himself obliged to study outside circumstances, such as the practice of the decorator, the paper-manufacturer, and the architect. It was pointed out above that little is known about the reflecting powers of the wall papers normally employed. It may be said that even less seems to be on record regarding the reflecting power of different qualities of paper. And in many cases the contrast between the type and its surroundings is still further reduced by the black from the other side of the paper showing through.

School Rooms, etc.—A great deal has been said recently about the necessity for good illumination in school-rooms. The duties of the school medical officer are already held to comprise the examination of the natural and artificial lighting, but in order that any valid record of the conditions can be preserved it is essential that simple and reliable methods of measurement should be available. The chief requirement in an instrument of this class is portability, and, provided it is reliable in action and not liable to get out of order, an excessive degree of accuracy is seldom called for. It may be pointed out, however, that for such work it is often desirable to study surface-brightness rather than illumination, and it is equally important to be able to measure the illumination in any desired direction.

For example, although a knowledge of the horizontal illumination on the desks is useful, this does not always describe the exact conditions under which the child reads. He may lift the book at such an angle that he normally

receives but a fraction of the horizontal desk-illumination. Or it may be that, unfortunately, there are obstructions which throw a shadow on the page as usually studied, or the paper of the book may be of an inferior quality, which only reflects a small quantity of light. Therefore, the most perfect conception of the surface-brightness he enjoys would be obtained by looking through a photometer over the child's shoulder so as to read with the apparatus exactly that brightness which his eye would receive.

When one comes to consider illuminated diagrams and blackboards we are again concerned mainly with surface-brightness. For what really enables a child at the back of the room to distinguish the writing on the board is *the contrast* between the blackboard and the chalk. And the blackboard is sometimes not black but grey, while the chalk is only a very indifferent white. An attempt was recently made by the authors to ascertain the degree of contrast between a patch of white chalk and the blackboard-surface in a class-room, at a certain technical college. It proved to be only 1 to 10. Naturally, under these conditions it becomes difficult for detail to be seen at a distance, and it might be found that the production of a better dead black surface for blackboards would be even more influential than increasing the illumination. It has likewise been computed that in few pictures does the painter succeed in securing a greater contrast of light and shade than 1 to 30, and it was probably this circumstance that led one celebrated artist of the past to attempt to deepen his foreground by pasting on black paper!

(The same thing may be observed in connection with porch-lighting. One of the main intentions of the light in this case is to illuminate the threshold, and so prevent the possibility of anyone entering or leaving the building stumbling over the step. If the step and the

path have about the same tint, a very considerable illumination may be needed to render the one quite distinct from the other. On the other hand, if the step is newly whitened, a much lower illumination might answer. In fact, under practical conditions, the efficiency of any lighting installation for the purpose in view is very materially affected by this question of contrast. It has been often remarked that a very bright object appears much more glaring when exposed to the eye with a dark background, and it has therefore been suggested that, in order to avoid glare, it would be desirable to specify both a maximum intrinsic brilliancy and a maximum contrast between a source and its surroundings. In the same way one might suggest that in order to secure *distinctness* a certain *minimum* degree of contrast is required. The conditions in this respect could be very readily studied by means of a surface-brightness photometer. It could also be applied to examine the depth and nature of the shadows cast by different systems of lighting—a matter which is often of considerable consequence as regards the æsthetic effects of illumination.)

Until one attempts to make measurements one would hardly credit the variations in daylight that occur in different parts of an average room—even in a room very liberally supplied with windows. At the technical college referred to above there is one class-room, 30 feet by 25, with high windows all the way down the left-hand side (facing the street). Yet at 5 o'clock on a September day the illumination on the best lighted desk was 10 foot-candles, while the worst seat received only 0.6. Moreover the illumination of the blackboard varied from 5 foot-candles at one end to only 0.5 at the other.

Illumination in Tramcars, Railway Carriages, etc.—There are few instances of wider variations in lighting conditions than those

which seem to prevail in railway carriages and tramcars. As yet there does not seem to be any regular standard adhered to. Nevertheless, it may safely be said that the nature of the illumination is often a powerful inducement to travellers to select one route in preference to another. One of the authors was even assured recently by a gentleman who regularly travels up and down to Brighton that he sometimes deliberately waits for a later and slower train, in preference to an early fast one, because he has a great deal of reading to do on the journey, and better facilities are provided as regards illumination in the latter case.

The illumination provided in the tubes and underground trains of London varies very greatly. In one of the tubes by which the authors habitually travel, the horizontal illumination is usually between 1.1 to 1.3 foot-candles while the train is at rest, and 0.7 to 0.9 foot-candles when the train dives into the tunnel. This value has been repeated so often that one could almost check the instrument by a reading on this tube.

In other cases readings between 2 and 3 foot-candles were obtained while in one comparatively short journey, on another route, the illumination fluctuated between 0.2 and 0.8 foot-candles. Perhaps the most remarkable instance of poor illumination was a tram in which the lights were constantly going out, and the illumination danced about between 0.1 and 0.2 foot-candles. Yet the illumination in the next tram-car that passed along the route was over one foot-candle.

There is also a waiting-room on a well-known suburban railway junction in which it is apparently impossible to secure a higher illumination on one's newspaper than 0.28 foot-candles. Yet one very often sees a number of people in this room trying to read. It may be added that in few suburban stations is there any attempt to provide special illumina-

tion for the time tables outside. In this case the surface-brightness of one immediately opposite a lamp was 0.3 foot-candles; but another series a little further along yielded a result of only 0.07. Some criticism might also justifiably be levelled at the illumination of the clocks in some of the tubes. These not infrequently appear jet black against a white reflecting surface, or with a background of arc lamps. Actual measurement revealed that the surface-brightness in two cases was 0.2 and 0.03 foot-candles respectively.

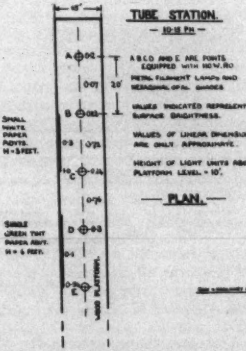
TUBE STATION.
— 10-18 Ft —
A B C D AND E ARE POINTS EQUIPPED WITH 100 W. 250 METAL FILAMENT LAMPS AND HEMISPHERICAL OPAL SHADES
VALUES INDICATED REPRESENT SURFACE BRIGHTNESS.
VALUES OF LINEAR DIMENSIONS ARE ONLY APPROXIMATE.
HEIGHT OF LIGHT UNITS ABOVE PLATFORM LEVEL = 10'.
— PLAN. —
SMALL WHITE PAPER ADVTS. H = 3 FEET.
SMALL GREEN TINT PAPER ADVT. H = 6 FEET.
LOAD PLATFORM.

FIG. 7.

It will be seen, therefore, that although considerable progress has recently been made in the lighting facilities on many railways, notably by the enterprising use of illuminated notices and signs, there are still possibilities for considerable improvement. It may be added that the distribution of illumination over the platform is often very uneven; observe, for example, the distribution of platform light over the platform of the tube station

shown in fig. 7. It should be stated that these figures refer to the surface-brightness of the actual platform itself. In mapping out distribution of illumination in such cases, it has been found a very simple and convenient plan merely to look at the platform and not to use a white surface. In this connection one might suggest that there is room for some discussion as to the exact functions of the light shed upon the platform in the railway station. One important function is certainly to enable the passengers to keep clear of the edge. It is obvious, however, that quite as vital, or perhaps an even more vital matter than the supply of abundant light—is the con-

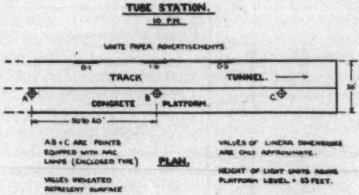

FIG. 8.

trast of light and shade between the platform and the rail-bed. If both have about the same reflecting power, no amount of light would enable the line of demarcation to be really distinct. The actual surface-brightness in such cases seems to vary enormously. In one of the tubes the surface brightness of the platform edge was 0.2 foot candles, and of the line itself 0.02. On another railway the contrast proved to be about the same, but the actual surface-brightness was about 0.01 and 0.001 foot-candles respectively!

One other matter to which reference may be

made is the light thrown upon advertisements. A typical illustration of this is fig. 8.

It will be seen that those placards which happened to face an arc lamp enjoyed a surface-brightness of about 1 foot-candle, while other neighbouring advertisements only received about one-tenth of this value.

These few figures seem to the authors to show that there are still many points which deserve closer attention than they have yet received, and on which there seems as yet to be no general understanding. Possibly this is partly due to the fact that, with the exception of several well-known papers, some years ago, on the subject, very few actual measurements of the illumination seem to have yet been published. It may be expected that by the aid of a more general use of measuring instruments, and the preservation of definite records of conditions of illumination, it will be possible to come to some better agreement as to what is needed.

In presenting these figures regarding the conditions of illumination on tramcars and railways, the authors do not, of course, mean to convey that the lighting in these circumstances is especially unsatisfactory, nor to leave it to be inferred that there are not many other equally striking examples of uncertainty as to what constitutes adequate lighting. No doubt a complete study of the conditions in streets, schools, factories, etc., would reveal similar differences and deficiencies. This subject has been selected for special treatment merely because one's daily journeys to and fro in tramcars and railway carriages afford exceptional opportunities of making comparisons.

As stated above, the idea of the authors in devising this instrument has been, first and foremost, to provide a compact and portable apparatus for the measurement of surface-brightness in any plane. In most practical cases, such as those to which reference has just

been made, great accuracy is not expected. Nevertheless, special care was taken in the design of this instrument in the hope that it might also be found useful for many laboratory purposes, in which the very highest high precision necessary for standard work was not demanded. It is somewhat curious, as Mr. Trotter has recently remarked, that in much work in photometrical laboratories there has been no mean between extremely elaborate experiments and the use of somewhat primitive and antiquated apparatus. In many cases a student wastes a considerable amount of time in making calculations which might be avoided, and sometimes conceives an aversion to all photometrical experiments, as something unreliable and tedious, as a result.

An instrument, such as this, in which a uniform scale is employed, saves a considerable amount of time, because the scale is direct reading, and is always available for relative results; the long calculations based on the inverse square law, usually associated with work on a photometric bench, are avoided. Probably, therefore, an instrument contrived on this system, which can be used to measure either surface-brightness, illumination, or candle-power, would often be a valuable adjunct in the photometrical laboratory, even though it could hardly be expected to yield the same degree of accuracy as a long photometric bench. On the other hand, its portability and compactness is often an advantage. A photometric bench takes up a great deal of room, and is not readily transferred from one part of the room to another. The instrument should, therefore, be serviceable for fundamental experiments by students, on lamps, shades, or laws of reflexion, as well as for measurements of illumination. The apparatus need not always be held up by the observer. During an experiment it should be mounted on a short tripod. One type of experiment for which it seems particularly well suited is the

study of the absorption powers of solutions and glasses and the reflecting power of different surfaces.

It might also be applied with advantage to obtaining polar curves of light distribution, since it can readily be pointed at a lamp, so as to receive, vertically, the light from any desired direction. Another possible method of applying the instrument for this purpose would be to fit it with an angle tube, terminating in a ground opal plate, which can be rotated to any

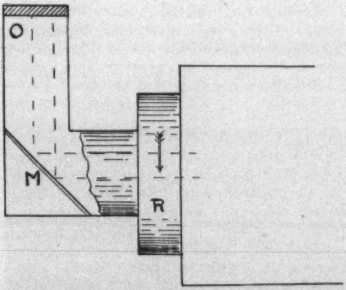

FIG. 9.—DIAGRAMMATIC VIEW OF GRADUATED ANGLE-TUBE FOR OBTAINING POLAR CURVES OF LIGHT-DISTRIBUTION.

desired angle without the position of the instrument being affected. A diagramatic sketch of such a tube is shown in fig. 9.

There are many other purposes for which simple instruments of this type could be used, but enough has perhaps been said to show the need for portable apparatus of this kind as a guide in practical problems of illumination.

In conclusion, the authors wish to express their appreciation of the care and trouble taken

by Messrs. R. & J. Beck, Ltd., the makers of the "Lumeter" instrument, and their special indebtedness to Messrs. Conrad and William Beck, who have suggested many valuable modifications now embodied in the design. Acknowledgment is also due to the courtesy of the Holophane Co. in granting permission for tests of the instrument to be carried out in their laboratory.

And in conclusion the authors would like to record their grateful appreciation of the encouragement and interest in this work of Mr. L. Gaster, and their consciousness of the benefit derived from the recent discussions of the Illuminating Engineering Society on the subject of "Glare, its Causes and Effects," and "The Measurement of Light and Illumination," which were largely instrumental in suggesting the value of an instrument of this kind.

ON OPHTHALMIC INSTRUMENTS.

By H. S. Ryland.

Abstract of a Lecture delivered 10th November, 1910.

Ophthalmological instruments may, in general, be divided into five sections, viz., those for measuring curvature, the refractive condition, the muscular condition, the field of vision, and for internal examination.

Dealing with these in order, we have first the ophthalmometer, for corneal measurements, and kindred instruments. In general principle, the ophthalmometer is dependent upon the relation between magnification and radius.

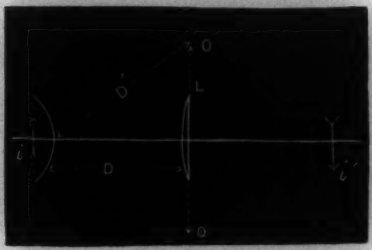

Fig. 1.

In † fig. 1, if A be the section of a convex mirror, O an object at distance D, and i the virtual image formed in the mirror, the magnification and radius are connected by the equation $1/m = 1 - 2D/r$, which, if we write O/i for $1/m$ becomes $r = 2Di/(O - i)$.

†Where O is large compared with D, the distance D' must be used.

In general ophthalmometric practice, however, i is so small compared with O that it may be neglected in the denominator, and the equation becomes $r = 2Di/O$.

In order to be able to observe, and consequently measure the size of, the image i, an objective L is so placed that one of its symmetrical points coincides with i, a secondary image i' being formed at the other. Since i' is a real image it may be received upon a screen or observed by means of an eyepiece. Further,

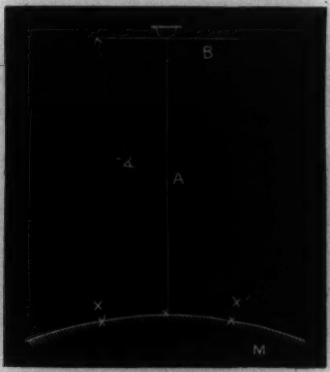

FIG. 2.

being of the same size as i, a measurement of i' is equivalent to a measurement of i. The experiment demonstrated will show how simple this measurement is with an inaminate mirror, but when the mirror is in a state of constant movement it is a very different matter.

While at this stage, I should like to draw your attention to a simple means of measuring the radius of a mirror when more accurate means are not available.

Let M., fig. 2, be a mirror, either convex or concave, and A a pencil or rod of known length. Then, if a rule or object of known length B, be supported by the finger on the top of B, the size of the image can be determined by marking its extremities as appearing in the mirror by two ink spots X and X′. Then, in our equation $A=D$, $A'=D'$, $B=O$, and X to $X'=i$.

By this means, quite reasonably accurate results may, with care, be obtained.

To return to the ophthalmometer. In order to overcome the disturbing effect of the ocular movement, the optical dividers are resorted to. This is a system of prisms or other means of dividing the beam from i, fig. 1, into two parts, and forming two images in the same plane, but with a definite separation at the position i, fig. 1. If the size of the image i is such that its two images separate a distance equal to its own length, the lower extremity of one will be in contact with the upper extremity of the other. In this case the size of the image is the amount of separation produced by the doubling system.

There are two possibilities for measuring images of varying size, one to vary the size of the object so that the size of the image fits the separation (this is the more general method); secondly, to vary the doubling to fit the image. This method is seldom adopted, as, in general, a small error in setting means a large error in final result. This difficulty of setting has been overcome by Mr. J. H. Sutcliffe, in his keratometer. I have only described here the general principles underlying ophthalmometers.

I wish now to describe a principle due, I believe, to Dr. Drysdale. This method consists of forming an image upon the surface of the mirror by means of a convex lens. This light from this image, after returning through the lens, will form an image coinciding in position with the object, as if the mirror were a plane surface.

If now the mirror be moved towards the lens until the image formed by the lens coincides

with the centre of curvature, the image, after passing through the lens, will again coincide with the object, since the light from the lens is meeting the mirror surface normally.

An attempt has been made in America to utilise this principle for corneal measurement by mounting a microscope with vertical illuminator upon a slide, and racking in towards the eye to pick up the two images, the distance between them, of course, being the radius. But through the subject refusing to remain steady, it is impossible to make the measurements.

After some experimenting, I have succeeded in overcoming this difficulty by using a lens of large diameter, A, fig. 3, and a second lens of

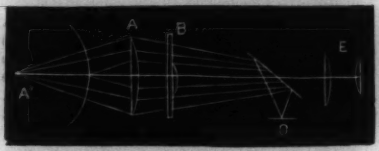

FIG. 3.

smaller diameter, B, mounted upon a plane parallel glass plate. The effect is that light passing through that part of A not covered by B will focus at A′, while that passing through A and B will focus at B′. When the images of these images coincide at E, the corneal radius is the distance from A′ to B′. By using an illuminated ring, O, as object, and noting the difference in the distance A′ B′ for coincidence at different parts of the ring, we may measure differences of corneal radii.

I have applied for provisional protection covering this method, but it remains to be seen if it is of practical value.*

To turn to section 2, we have optometers of

*A finished model has since been made which appears to work well.

various types, and the retinoscope. The ophthalmoscope I will leave for section 5.

The general idea of the optometer is to measure an artificial punctum remotum produced by a convex lens. Various applications of this general idea have been made, one of the most important being the combination with the convex lens of a Scheiner double pinhole or slit, as in the Thomas Young model. There is not time this evening to enter into the development of different types of optometer. I will, therefore, show the difficulties of optometric measurement. In the first place, it is very difficult for an average person to say when an image is sharply defined, and, in the second, the fact that he knows that he is looking at a near object will cause him to accommodate.

I have made an attempt to overcome this defect by using two convex lenses of the same focal length, adjustable for separation, but normally in afocal adjustment. An emmetrope using the instrument in this position will see the image of a distant object, whose size is that of the object, but inverted. To prevent doubt as to definition, a double slit is used in contact with the lens used as an eyepiece. Hence, when the patient sees a distant object sharply defined *and single*, the position of the lenses will be a measure of the refractive condition.

By making the cap carrying the double slit rotable the refraction in different meridians may be estimated.

I would like to suggest that you term the effective accommodative power of the eye the range of accommodation, and the actual near point expressed in diopters, amplitude. I think it would fall in better with the natural meaning of the terms used, as well as simplifying the work.

The principle of the retinoscope is too well known to need description here, but I should like to point out that one of the great pitfalls for the beginner in retinoscopy is the spherical

aberration of the refracting system of the eye. The centre of the pupil will sometimes appear H, while the periphery is M. This will present little or no difficulty if the central shadow only be taken.

With regard to the ophthalmoscope, there is one thing I should like to call your attention to: the possibility of making it give you a larger illuminated field of view. I feel somewhat firmly convinced that before long we shall revert to the earlier form of ophthalmoscope, in which an unsilvered mirror is used.

Among other things, Mr. Ryland spoke of the possibility of using a Placido disc made of opal glass, divided into small squares instead of circles, and fitted with a corneal microscope for the determination of conical cornea. He also showed an instrument he had made for retinoscopy, consisting of a tube 1 meter long fitted at one end with a ring of lenses, driven by a star-wheel, to pass an eye-piece. The patient looked into the eye-piece while the operator at the other end was able to watch his shadow, and, by means of a milled head, bring whatever lens was required in front of the patient's eye without putting his retinoscope down.

The whole lecture was fully illustrated by experiments and blackboard demonstrations.

Discussion.

Mr. W. A. Dixey said he had been very much interested in Mr. Ryland's paper, which he thought was most suggestive. His illustrations of methods of employing different instruments had been of very great use. Perhaps not the least interesting parts of the lecture were his bold suggestions for new types of instruments, in regard to some of which one was inclined to feel quite hopeful as to their eventual development into something practical. Of course, when any new instrument was suggested, one always thought first of the objections to it. And in re-

spect of the new model of optometer, he was a little doubtful as to the practicability of the instrument described by Mr. Ryland, as, in estimating the refraction of an eye, one had to deal with the resultant of all the different curvatures in all the different meridians of the effective part of the eye. He was particularly interested in the mechanical retinoscope. Oddly enough, a little time ago, the same device was suggested to him, he being quite ignorant at the time that Mr. Ryland had made anything of that kind. The drawback in the model of Mr. Ryland, he thought, was the size of the aperture—barely $\frac{3}{16}$in. In his (Mr. Dixey's) model it was 1in., and even that was found too small in practice. To return to the optometer, he was very much taken with the application of Scheiner's double aperture to an optometer, and he should like to see it in practice. It seemed to him that it ought to do very simply and very exactly all that the optometer would do. Of course, Mr. Ryland was conscious that the optometer had its limitations. One of the points against it was that it confined the result to one zone of the pupil and the result was not the general effect of the whole of the lens, but just a measurement of the amount of refraction in the one zone, and that zone a marginal one, as was necessary from a consideration of the effective quality of the instrument

THE PRESIDENT (Mr. S. D. Chalmers, M.A.) thanked Mr. Ryland for his paper, and said he would like to add one or two comments. The principle used in the ophthalmometer could be applied with advantage to the measurement of curves of short radius, particularly for microscope lenses. It was preferable to use no doubling system but to place the lens on the stage of a microscope and then move either lens or microscope till the image of each end of the object in turn came on the cross wires; the relation between the object size and image size was then approximately the

same as that given by Mr. Ryland, and a more exact formula could be easily obtained. He (the President) had used the method to measure curves of as little as 4 mm. radius, and in this case considered an accuracy of 1% was easily obtainable.

MR. RYLAND replied to the points raised by the speakers, and he was accorded a hearty vote of thanks.

ON THE THEORY OF THE IRIS DIAPHRAGM.

By Cyril F. Lan-Davis.

Paper read on December 13th, 1910.

The words "diaphragm" and "stop," whatever was the original distinction between them, are now used indiscriminately by many writers. I propose, therefore, treating them as synonymous.

The earliest stops, probably dating back to the introduction of the telescope, were plain circles, generally pierced with round holes, which acted satisfactorily for single lenses. They were also used in the early photographic Petzval lenses, being then placed between the combinations.

Their obvious disadvantage, the trouble involved in changing them, was removed by the invention of Waterhouse stops. As you know, these are also separate metal pieces, which are inserted in a slot in the lens mount. This type is still largely used for portrait lenses, designed for studio use only, and for process lenses.

Their defects are their size and weight, and the inconvenience of carrying a number of separate parts. One method of removing this latter objection is to rivet the Waterhouse stops together at the top. Chevalier, in the middle of the last century, devised a method of hinging the stops to the lens mount.

Another early form of diaphragm was the rotating type. A flat plate is fastened inside the lens tube, and a circular plate is pivoted near its outer edge. Holes of different diameters are pierced in this plate, each of which in turn can be brought between the lenses. A spring catch controls the exact position of each. This

rotating form is excellent for wide-angle and stereoscopic lenses, where only a small range of apertures is wanted. It becomes bulky for large lenses, and cannot easily be made to give a sufficient range of apertures.

In these forms of diaphragm the actual opening could be circular, square, or any other shape, such as process workers use, but there was a sudden jump from one size to another. A form of adjustable stop with square openings was then invented by Noton, in 1856, and an adjustable circular stop patented by Pierre Maugey in 1858. The first consisted simply of two plates, with square openings cut at an angle of 45°, sliding together, and the second of a

FIG. 1. FIG. 2.

hollow rubber disc, the size of the hole in which was varied by stretching and relaxing the rubber.

I have made an adjustable form of diaphragm with square openings on the lines of Noton's diaphragm. It is worked by a rotating slotted plate, and at full aperture gives a circular opening, the shape of each leaf being as shown in fig. 1. This may be of use in process work.

I have been unable to find exactly when and by whom the iris diaphragm was invented. According to Charles Fabre's Photographic Encyclopædia, it was known to Niepce, and was

probably invented by C. Kircher. Chevalier, in 1840, received a medal for exhibiting one in Paris. An iris of six leaves is shown in an illustration to Monckhoven's Photographic Optics, published in 1867, and a patent was granted to Mr. W. J. Lancaster for a specific form of iris diaphragm in 1886. The patent claims of this specification were, however, amended, to show that it was not a master patent. Mr. Brothers states that two rings of metal, with rubber between, were first used to cover jam jars, and that this developed into the iris.

The iris diaphragm was early used for the substages of microscopes, and is now generally adopted for photographic and other lenses. So far, however, as I can trace, there has been no mathematical explanation of its working, at all events, in England.

In mounting a lens it is necessary to see that the diaphragm used is large enough to transmit the full beam of rays, and that this can also be varied to give apertures of $f/32$, $f/45$, $f/64$, etc. I have been endeavouring recently to standardise some of the tube sizes of Dallmeyer lenses, and have found that the first difficulty is to obtain an iris which would give the very large apertures required for Dallmeyer Patent Portrait lenses, and also the smallest apertures necessary for Dallmeyer Stigmatic and wide angle lenses.

There appeared no method available for calculating these apertures. Previously a workman, at great trouble, worked out experimentally a set of leaves which gave fairly well what was required. At the same time, I was constantly met with the remark that one could not have both very large and small apertures—it was expecting too much in this ill-contrived world. I endeavoured, therefore, to work out the theory of these diaphragms, and obtained a series of formulæ which should allow future iris diaphragms to be rapidly calculated.

The Iris Diaphragm.

Circular Apertures.

As shown by a number of examples which makers have kindly lent me, the modern iris consists essentially of

a fixed plate pierced with holes at its circumference,

a set of leaves with pins projecting from each end on opposite sides,

a circular tube or shell generally made in one with the lower plate,

a rotating plate with radial slots fitted to turn freely inside the shell, and held in this shell by a clamp ring or similar device,

a milled ring or handle on the outside of the shell, connected by a pin with the slotted plate.

As is well known, one end of each leaf is fitted in the fixed plate and the other in the rotating plate. Rotation of the iris handle then opens and closes the iris. I propose to ascertain the best form of this construction. In each case I consider that the exterior of the iris is circular, and that the apertures are also circular. The boundaries of each leaf must therefore be circles, one the same diameter as the exterior of the iris, the other as its full aperture, and the leaf must be roughly the shape shown in fig. 2.

Position of Lower Pin, *i.e.*, Pin in the Fixed Plate.

In fig. 3, the larger circle ABB′ is the exterior of the iris, the smaller circle PGG′ is the full aperture of the iris, and C′ is their common centre.

The most important case to consider is now that the iris should close from full aperture PGG′ to nothing. That is to say, the leaf PABG must rotate round some point as centre until its edge PG passes through the centre C′.

Considering that the diameter of the pin is small, we may say that the leaf PABG may rotate round any point of itself. Clearly, how-

ever, this point should be at one or other end. At any other position the leaf would be unable to turn.

If we take the two extreme cases, the pin on circle PGG′, that is, the pin at opening, and the pin on circle ABB′, that is the pin at the extremity of the leaf, the other cases will lie between.

In fig. 3 the lower pin P is at opening; in fig. 4 the lower pin A is at the extremity of the leaf.

In fig. 3, let P be the point around which the leaf PABG rotates, and let its new position, with edge passing through C′ be PA′B′G′. Now the end B of the leaf is carried round by the slot in the rotating plate, which is the shape of the zone BB′GG′—*i.e.*, this point B must move within zone BBGG′. Therefore we must make PB as long as possible consistent with allowing the edge PG of leaf PABG to pass through C′.

i.e., in the new position PA′B′G′, B should lie on outer circle ABB′. Then PB′ is length of leaf.

Let PB′ meet circle PGG′ at G′. Draw C′K ⊥ PB′ meeting circle PGG′ at K, ∴ KG′ = KP.

Then centre of circle A′D′B′ lies on C′K,

∴ centre of circle PC′G′, which is concentric with circle A′D′B′, lies on C′K, and by construction this lies on circle PGG′,

∴ centre is at K, and PA′B′G′ is new position of leaf, and C′K is bisected at E.

Also △ C′K̂P is equilateral, ∴ C′P̂K = 60° and C′P̂G′ = 30°. In △ˢ C′GP, C′G′P, CP is common, C′G = C′G′ and PG = PG′,

∴ G′P̂G′ = 2 C′P̂G′ = 60°.

Again, PÂ′K = ½ PC′K = 30°. We can therefore work out the following results by trigonometry.

MAXIMUM APERTURE.

If we consider what is the greatest opening which an iris can have consistent with its

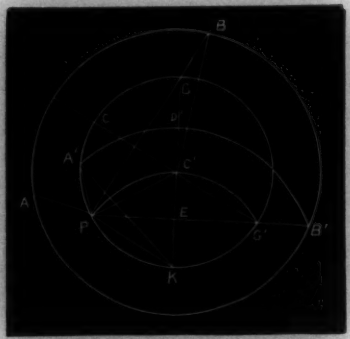

Fig. 3.

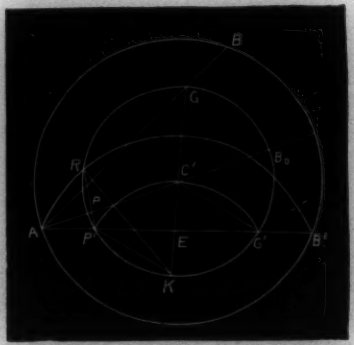

Fig. 4.

LOWER PIN AT OPENING (FIG. 3).

Iris diameter $= 2\,C'B' = 2a$ (say)

Iris aperture $= 2C'G' = 2b$ (say)

Length of leaf $= PB' = PE' + EB'$

$= \frac{1}{2}\ (b\sqrt{3} + \sqrt{4a^2 - b^2})$

Minor length of leaf $= PG' = b\sqrt{3}$

Greatest width of leaf exposed $= PA'$

$= \frac{1}{2}\ (a\sqrt{3} - \sqrt{4b^2 - a^2})$

Width of leaf $= a - b$

Distance between pinholes $= PA' =$

$= \frac{1}{2}\ (a\sqrt{3} - \sqrt{4b^2 - a^2})$

If $P\hat{C}'A' = 2\alpha = 2\sin^{-1}\dfrac{a\sqrt{3} - \sqrt{4b^2 - a^2}}{4b}$

Number of leaves required $= \pi/\alpha$

$= \dfrac{4b\pi}{a\sqrt{3} - \sqrt{4b^2 - a^2}}$ nearly

Travel $= BB' =$ length of leaf

$= \frac{1}{2}\ (b\sqrt{3} + \sqrt{4a^2 - b^2})$

LOWER PIN AT EXTREMITY OF LEAF (FIG. 4).

$= 2\,C'B' = 2a$ (say)

$= 2\,C'G' = 2b$ (say)

$= AB'$

$= \sqrt{4a^2 - b^2}$

$= AG' = \frac{1}{2}\ (b\sqrt{3} + \sqrt{4a^2 - b^2})$

$= P'R$

$= \frac{1}{2}\ (a\sqrt{3} - \sqrt{4b^2 - a^2})$

$= a - b$

circumference $\div$ number of leaves.

$= \dfrac{a}{2b}(a\sqrt{3} - \sqrt{4b^2 - a^2})$

If $P\hat{C}'R = 2\alpha = 2\sin^{-1}\dfrac{a\sqrt{3} - \sqrt{4b^2 - a^2}}{4b}$

$= \pi/\alpha$

$= \dfrac{4b\pi}{a\sqrt{3} - \sqrt{4b^2 - a^2}}$ nearly

$= (b/a)\sqrt{4a^2 - b^2}$

closing, we see that the leaf must be sufficiently short to allow its inside edge to come to the centre of the iris, and at the same time the upper pin must at all times be in the zone BB′G′G (fig. 4). That is to say, in the limiting case, the circle with A as centre and B as radius, which is the path of B, must touch the circle P′GG′.

That is, the length of the leaf AB must be equal to the radius AB_0 of such circle, from which condition we can work out the results in each position of the lower pin.

Lower Pin at Opening (fig. 3).

For maximum opening, the condition is then that $2C'P = PB'$,

$$i.e.,\quad 2b = \tfrac{1}{2}\ (b\sqrt{3} + \sqrt{4a^2 - b^2})$$

$$i.e.,\quad 4a^2 = b^2(20 - 8\sqrt{3})$$

$$a^2 = b^2(5 - 2\sqrt{3}) = 1.536\,b^2$$

$$\therefore b = .807a \text{ and } a = 1.239b.$$

$$\text{Number of leaves required} = \frac{\pi}{\alpha}$$

$$\text{where } \alpha = \sin^{-1}\frac{a\sqrt{3} - a\sqrt{(.614)(2.614)}}{3.228a}$$

$$= \sin^{-1}\frac{1.732 - 1.267}{3.228} = .145.$$

$$\text{Number of leaves} = \frac{3.142}{.145} = 21.66 = 22.$$

$$\text{Width of leaf} = a - b = .193a.$$

$$\text{Separation of pinholes} = PA' = .233a,$$

$$\text{or } = 2b\sin\frac{2\pi}{44} = .284b = .229a.$$

LOWER PIN AT END OF LEAF (FIG. 4).

For maximum opening, the condition is

$$C'A + C'P' = A'B'$$

i.e., $a + b = \sqrt{4a^2 - b^2}$, from which

$$a = 1.215b, \text{ or } b = .823a.$$

Again,

$$\alpha = \sin^{-1}\frac{a\sqrt{3} - a\sqrt{(.646)(2.646)}}{3.292} = .129.$$

$$\therefore \text{Number of leaves} = \frac{3.142}{.129} = 24.3 = 25.$$

Width of leaf $= a - b = .177a$.

$$\text{Separation of pinholes} = 2a \sin\frac{2\pi}{50} = \tfrac{1}{4}a.$$

FIG. 5.

CLOSING TO A KNOWN APERTURE.

Sometimes it is not necessary that the iris should give very small openings. If it is only desired to stop down to opening, radius C'D' =

c, as in fig. 5, we can also work out a set of formulæ. I here only work out the case of the lower pin at opening. The results for the pin at extremity of the leaf have been worked out in a similar way.

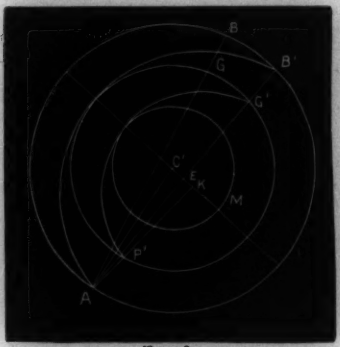

FIG. 6.

LOWER PIN AT OPENING (FIG. 5.)

By hypothesis, circle D′M touches edge PD′G′ of leaf,

∴ centre K of circle PD′G′ lies on C′D′ at distance b from D′, and C′D′ bisects the common chord PG′ of circles PGG′, PD′G′.

Also from $\triangle^s$ C′PE, KPE,

$$C'E = EK = \tfrac{1}{2}C'K = \tfrac{1}{2}(b-c).$$

$$\therefore\ PE = \sqrt{C'P^2 - C'E^2} = \sqrt{b^2 - \frac{(b-c)^2}{4}}$$

and $$EB' = \sqrt{C'B'^2 - C'E^2} = \sqrt{a^2 - \frac{(b-c)^2}{4}}$$

We have then the following results:

Lower Pin at Opening (fig. 5).	Lower Pin at Extremity of Leaf (fig. 6).
Length of leaf $= PB' = PE + EB'$	$= AB'$
$= \frac{1}{2}\{\sqrt{(2a-b+c)(2a+b-c)} + \sqrt{(b+c)(3b-c)}\}$	$= \sqrt{(2a-b+c)(2a+b-c)}$
Minor length $= PG'$	$= AG'$
$= \sqrt{(b+c)(3b-c)}$	$= \frac{1}{2}\{\sqrt{(2a-b+c)(2a+b-c)} + \sqrt{(3b-c)(b+c)}\}$
Travel $= BB' = 2\,PB' \sin C'\hat{P}B'$	$= BB'$
$= \frac{b-c}{2b}\{\sqrt{(2a-b+c)(2a+b-c)} + \sqrt{(b+c)(3b-c)}\}$	$= \frac{b-c}{a}\sqrt{(2a-b+c)(2a+b-c)}$
Width of leaf $= a-b$	$= a-b$
Number of leaves $= \frac{2b\pi}{a-b}$ approximately, as	$= \frac{2b\pi}{a-b}$ approx.

$a-b$ is generally small.

If $c=o$, these results re duce to those already found.

DISCUSSION OF RESULTS.

The aims in designing an iris diaphragm are, of course, to obtain the following:

The greatest aperture consistent with retaining the smallest opening. A long scale with equal divisions. Light weight, ease in working, freedom from failure, and reasonable cost.

The portions which can be varied are:—The position of the pin in the fixed plate, the position of the pin in the slotted plate, the width of the leaf, the length of the leaf, the number of leaves, and the angle of the slots.

It is of no importance whether the slotted plate or the pierced plate be moved over the scale. Most makers prefer the first method, but some have adopted the second.

From the calculations for maximum opening we see that the pins should be placed at the opposite extremities of the leaves, and that twenty-five such leaves are required. The maximum aperture then obtainable is 82% of the total diameter of the iris. No increase in the number of leaves will help to give a greater opening.

If, however, the smallest apertures are not required, the number of leaves may be reduced, or an even greater aperture obtained.

The position of the pins is here all important. As we see from the formulæ, the lower pin at opening gives a much greater length of travel.

Generally, to obtain a large aperture, it is necessary to have the upper pin which moves in the slotted plate at the extreme end of the leaf. When, however, it is not necessary, a small increase in the length of the scale may be obtained by placing this pin on the end of the inner edge.

I have here a pair of diaphragms, containing an equal number of leaves, and each leaf of the same width. In the first, the pins on each leaf are set at opposite extremities. In the second, each pin is set as near the opening as possible. Both give an equal range of apertures, but the

length of scale of the second is considerably greater than that of the first.

This depends, of course, how the apertures are calculated. For the f/Nos. scale of the Royal Photographic Society, the divisions are wide apart for apertures $f/1$, $f/2$, etc., but rapidly decrease until the markings are inconveniently crowded for $f/32$, $f/45$, $f/64$, etc.

In fig. 7, P is the lower pin, as usual. We may take the upper pin, which moves in the

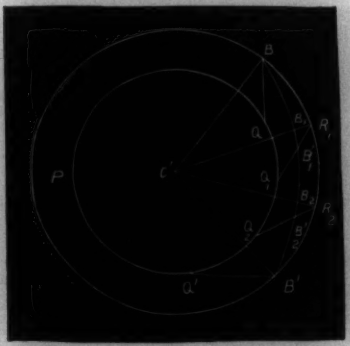

FIG. 7.

slotted plate at the extreme end of the leaf at B. Then the travel of B is over the arc BB_1B_2B' of circle with centre at P, and the scale is the arc BR_1R_2B'.

If the slots are cut radially as $C'B$, $C'R_1$, $C'R_2$, $C'B'$,

then div. of scale BR_1 corresponds to travel BB_1

" R_1R_2 " " B_1B_2

" R_2B' " " B_2B'

and in this case the scale divisions are widest at the beginning, and become rapidly smaller.

If, however, the slots are cut at a constant angle to the radius, as BQ, R_1Q_1, R_2Q_2, B′Q′, the div. of scale BR_1 corresponds to travel BB'_1

,, R_1R_2 ,, ,, $B'_1B'_2$

,, R_2B' ,, ,, B'_2B'

i.e., for equal divisions of the scale compared with the last case, the travel is less at the beginning and greater at the end.

I have therefore been able to construct an iris in which the division

for *f*/6.3 to *f*/8 = division *f*/32 to *f*/45,

,, *f*/8 ,, *f*/11 = ,, *f*/22 ,, *f*/32,

,, *f*/11 ,, *f*/16 = ,, *f*/16 ,, *f*/22.

By altering the angle of the slot we can then increase or decrease the widths of the divisions without altering the total length of the scale. Unfortunately, however, the direction of the slot tends towards the tangent to the arc of travel of the pin. The iris in consequence is liable to give different apertures for the same position on the scale. Only small deviations of the slot from the radial direction are therefore practical.

The upper pin can be shifted quite close to the lower, and the leaf shortened to nearly half its length. A large aperture can, however, not be obtained, and, perhaps more important, the unsupported leaves are not rigid, and do not give the same aperture at each setting. The system answers admirably for shutters, where only the aperture at the beginning and end need be constant.

The actual construction of irises varies very much, as the patterns here will show.

In the latest Dallmeyer patterns the pierced plate is made in one with the lens mount, and the slotted plate is rotated by a milled ring. Each slotted plate is actually cut by a milling tool, as this allows the full width of the ring to be used. When the plates are stamped there is always a portion of the width lost. The leaves are made of brass or steel, finished black, which keep their colour very fairly. The vulcanite

often used is liable to get out of order when exposed to heat.

Perhaps you may care to inspect the examples here, which have been kindly loaned to me by various makers.

ON THE RESULTS OF SOME TESTS ON PHOTOGRAPHIC SHUTTERS AT THE NATIONAL PHYSICAL LABORATORY.

By Mr. T. Smith.

Paper read 12th January, 1911.

The tests referred to in this paper were taken by the method and with the apparatus fully described in a paper read before the Physical Society.* The time is measured by receiving on a falling photographic plate a focused beam of light, which is made by electrical means to vibrate from side to side in a horizontal plane at a known uniform rate. When the plate is developed the time record appears as a sine curve, in which the interval of time taken by the plate to move through the distance separating two successive vertices of the sine curve is a definite fraction of a second. In the tests to be discussed these intervals of time were 1/25th, 1/50th, 1/300th, 1/1,000th, and 1/2,000th of a second.

The simplest method of testing is to place the shutter in the path of the vibrating beam of light, so that the beam can only reach the plate while the shutter is open. To obtain the duration of the exposure it is then only necessary to count the number of vibrations recorded on the plate. Fig. 1 shews an example of a plate in which this method was used. Each complete vibration took 1/300th second. The numbers of

*"On a Method of Testing Photographic Shutters," Proc. Phys. Soc. Vol. XXII., p. 782. Phil. Mag. Nov. 1909.

vibrations recorded are 18.6, 5.1, 4.5, and 6.1. Hence the durations of exposure in the four cases shewn were .062, .017, .015, and .020 of a second respectively.

By this method we can evidently only obtain the total length of time for which the exposure lasted. For many purposes this is not enough. It is desirable to be able to see what the shutter is doing at every instant of the exposure. A shutter may give exactly the duration of exposure which it is stated to give, and yet be a very unsatisfactory instrument. It may, for instance, not open to its full aperture, or it may

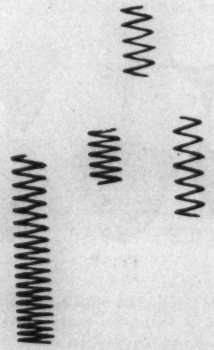

Fig. 1.

take a very considerable proportion of the time of exposure in opening and closing; in either of these cases under-exposure would very likely be the result.

To obtain this further information the vibrating beam of light is allowed free access to the photographic plate (*i.e.*, the beam does not pass through the shutter), and a separate record by the side of this time measuring sine curve shews

what the shutter is doing. Figures 2 and 3 (taken from the paper mentioned above) shew the method followed. Fig. 2 shews a series of stages in the opening of a shutter; to a particular length of the white line (which is really a slit placed close to the shutter leaves), there corresponds a definite area of the shutter opening. Fig. 3 shews for every instant of the exposure the length of the white line of fig. 2; the time is measured vertically by the aid of the vibrating light, and the length of the slit is the length of the portion of a horizontal line intercepted by the "shutter diagram." By combining the results derived from measurements of figs. 2 and

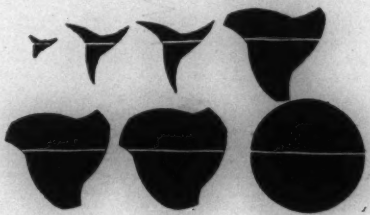

FIG. 2.

3 the area of the shutter opening admitting light to the plate is found for every instant of the exposure.

In testing focal plane shutters a similar method is followed. A very narrow illuminated slit is placed almost in contact with the shutter blind. As long as the blind opening is opposite this slit light reaches the photographic plate, forming a rectangular image by the side of the sine curve.

The results to be given refer to focal plane and to between lens shutters.

Before considering the results of the tests it is necessary to realise precisely what is meant by

the terms "speed" and "efficiency," as applied to these two types of shutters. It is convenient to denote by the "speed" of a particular shutter the length of an exposure defined in such a way as to depend solely upon the shutter; in particular, it should be independent of the aperture of the lens used. It may not in all cases be possible to do this, but it is possible in the case of focal plane shutters and of between lens

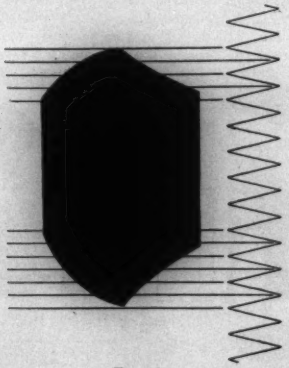

FIG. 3.

shutters which open and close at the centre. It happens, however, that if we adopt definitions of the "speed" which are independent of the aperture, by the speed of a between lens shutter we mean something very different from what must be understood by the same term when applied to a focal plane shutter. We define the speed of a between lens shutter of the type considered to be the interval of time between the instant when light first passes the shutter leaves and the instant when all light is cut off. In other words, by speed in this connection we

mean the total duration of exposure at the centre of the plate. From a practical point of view speed in this case is an indication of the rate at which an object may be moving without risk of producing an appreciable blurring of its image.

In the case of the focal plane shutter, the speed is defined as the interval of time taken by the opening between the blinds to cross the axis of the lens. This is a measure of the amount of light that will reach the centre of the plate during the exposure, *i.e.*, it indicates whether the plate will or will not be properly exposed; whatever the type of shutter, such a measure of

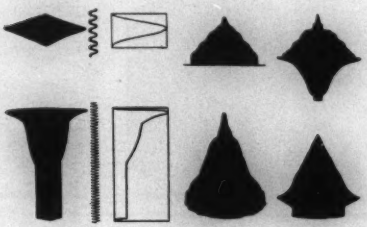

FIG. 4.

exposure is called the "equivalent exposure." We may define this as follows:—

The equivalent exposure is the length of time required to transmit to the plate an amount of light equal to that received during the actual exposure, when throughout this (equivalent) exposure we suppose that every part of the lens is effective which is effective at any instant of the actual exposure.

The exposures in all shutters are connected with the efficiency by the equation

Equivalent exposure = Total duration of exposure × efficiency.

From this it is evident that the "speeds" of focal plane and of between lens shutters are not

directly comparable. A case which may easily arise is one in which a focal plane shutter and a between lens shutter for a particular lens aperture have an efficiency of .5; the "speed" of the focal plane shutter may be .001 sec., and of the between lens shutter may be .002 sec. Under these circumstances the two shutters are equivalent to one another; in each case the length of time during which light reaches the centre of the plate would be .002 sec., and the equivalent exposure would be .001 sec. This

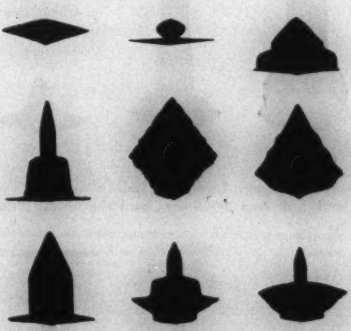

Fig. 5.

distinction must be borne in mind when considering the results of some of the tests.

The particulars which I now propose to give indicate in each type of shutter the highest speeds attained; the range of speeds, the general accuracy of the marked speeds, and the efficiency. This is followed by a short discussion of the factors that affect the efficiency, and some diagrams will be given illustrating a few cases of particular interest.

Focal Plane Shutters.

In ten typical shutters, of which only two were made to the same design, of the highest

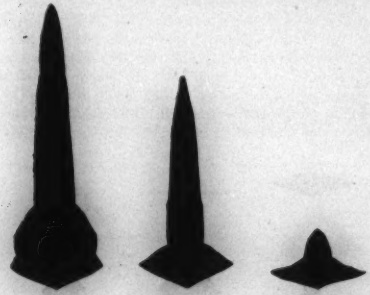

FIG. 6.

speeds obtained from the individual shutters, the highest was .0005 sec., and the lowest .006;

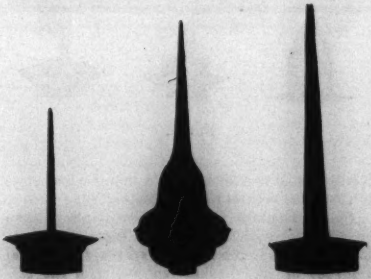

FIG. 7.

the mean of all the highest speeds was .0025 sec., four of the ten having their highest speeds

slower than this. Only two shutters attained as high a speed as .001 sec.

In the best case the highest speed was within 3 per cent. of its nominal value; in the worst case it was 3 times as long as it should have been —an error of 200 per cent. taking the marked speed as the basis of measurement. In some of these shutters the various speeds were denoted by a number, and no indication was given of the speeds they were expected to give. In the best case the mean error of all the marked speeds was 14 per cent., in the worst case 60 per cent., and the mean error of all marked speeds of all shutters was 43 per cent.

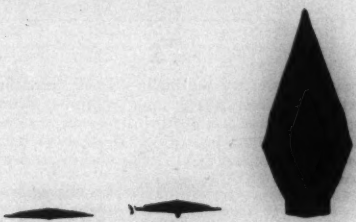

FIG. 8.

The longest exposures were between .089 sec. and .030 sec., with a mean value of .056 sec. The ratio of the lowest speed to the highest varied from 61 to 6.6 with a mean value of 33.

In half of these cases the alteration of speed was brought about solely by variation of the width of the opening between the blinds, and the speed was found to be strictly proportional to this width; in four cases the tension of the spring could be altered in addition to the previous adjustment, but the variation in the speed that could be obtained in this way was always small. The percentage shortening of the exposure obtained by changing from the least to the greatest tension varied from 17 to 35 per

cent., with a mean of 26 per cent. This is shewn in figs. 11 and 12, in which width of blind opening is measured horizontally, time verti-

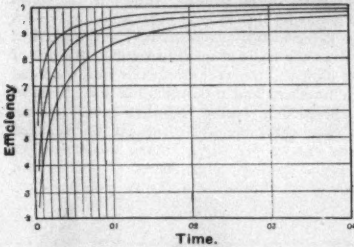

FIG. 9.

cally, and each of the inclined lines shews the relation between speed and width of blind opening when a definite spring tension is used. The fact that the lines are straight shews that the speed, with a definite spring tension, is proportional to the blind opening.

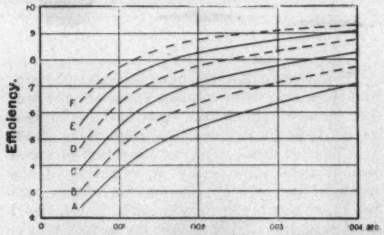

FIG. 10.

In one case a single operation both reduced the width of the opening and increased the tension of the spring.

The efficiency of a focal plane shutter is

$1 \div (1 + a\,b/f\,w)$, where a is the diameter* of the aperture used in the lens, b is the distance of the blind of the shutter from the emulsion of the plate, f is the distance of the back unit plane of the lens from the plate, and w is the width of the blind opening. In most cases f will not differ appreciably from the focal length of the lens, and a/f will be the aperture ratio of the stop in use. Thus to get the highest possible efficiency (*i.e.*, to make sure of getting fully exposed negatives of rapidly moving objects) the

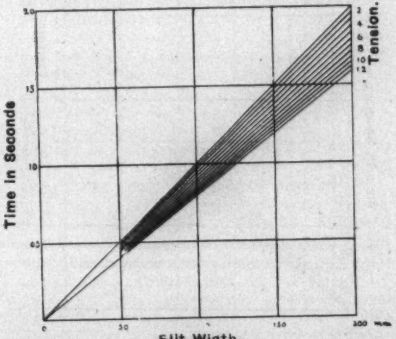

FIG. 11.

ratio b/w must be made as small as possible. The efficiency then can only be improved by reducing the separation between the plate and the blind or by increasing the rate at which the blind travels, and thus permitting a wider opening to be used. Figs. 11 and 12 shew that in the shutters tested no great difference in speed was obtained by increasing the tension. This

*This diameter is to be measured in the image space, and not by an actual measurement of the lens mount.

may be due either to the increase in tension being insufficient or to the near approach to the highest speed that the blind can attain with any tension with the construction adopted. It thus becomes of interest to see what velocities were obtained in these matters.

Considering only the velocities attained with the highest spring tension, the highest value found was 3.00 meters per second, the lowest

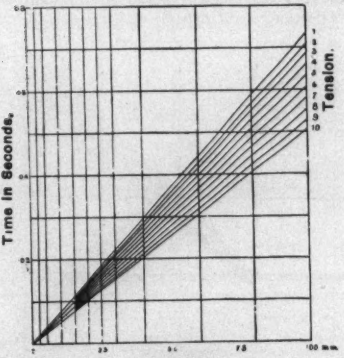

FIG. 12.

1.15, and the mean 1.84 meters per second. These values do not appear to be so high that they cannot be considerably improved.

The further consideration of the focal plane shutter may be conveniently postponed until some of the results obtained with between lens shutters have been considered, it will then be possible to make a comparison of the results, and to consider shortly the relative merits of the two types when either appears suitable for the work proposed.

The following results are taken from tests on twenty between lens shutters of sizes suitable for use with such modern lenses as are often fitted to half-plate or $5'' \times 4''$ cameras. Large between lens shutters have been excluded, as their speeds are slow in comparison with those of sizes in general use.

The highest speed (duration of exposure) reached by any of these shutters was .0023 sec.; the highest speeds attained by all the shutters were between this and .022 sec.; the mean of all the highest speeds was .0054 sec. In the the best case the highest speed was within 0 per cent. of its nominal value; in the worst case it was 2.2 times its nominal value. The mean error for all speeds was in the best case 15 per cent.; in the worst case 80 per cent.; and the mean error for all the shutters was 46 per cent. The longest exposures were between .17 sec. and 2.3 sec., with a mean value of 1.0 sec. The ratio of the lowest speed to the highest varied from 7.5 to 1000, with a mean value of 500.

In some of these cases the efficiency was determined. In the best case, with a highest speed of .005 sec., the efficiency was 54 per cent.; the efficiency with the lowest speed (.84 sec.) was 100 per cent., and the mean value for all speeds (average value .22 sec.) was 89 per cent. The mean results for all efficiency tests were highest (mean .0054 sec.) 50 per cent.; lowest (mean 1.0 sec.) 100 per cent.; average (mean .22 sec.) 80 per cent.

In the accompanying table the results of tests on between lens and focal plane shutters are arranged in a form in which direct comparison is possible.

Most of these between lens shutters appear to be fitted with a trip gear working in connection with a brake of some kind. In some cases this is in the form of a dash pot with air as the working fluid. In shutters of this type the leaves always open and close very quickly, and the greatest part of the exposure takes place with

the full aperture of the lens in use. At the fastest exposure there is no appreciable interval between opening and closing: in these shutters the efficiency at all speeds is good. In some cases, however, the trip arrangement is omitted, and the connection between the air brake and the leaves is direct. Thus the movement of the leaves enables an examination to be made of the way in which the brake works. The general result does not tend to increase belief in the reliability of the air brake. In some cases it works on suction, in others on compression; the former appears to be the more satisfactory, but in both the movement is liable to be jerky. This is well illustrated in figures 5 to 8, which are in marked contrast to figure 3. Figures 6 to 8 shew sets of three consecutive exposures, each set on the same shutter at the same setting, so that if the shutter were perfectly reliable the three diagrams forming any one figure should be identical in shape and size. In one case the exposure is about 10 times as long as on the two previous exposures. In another case the variation is very marked, but not as bad as in the previous case. In the third set the brake had a habit of sticking when the exposure should have been nearly over, so that the diagrams end in a long tail.

The movement of the piston is more likely to be discontinuous when the brake works on compression than when it works on suction. In the former case the first part of the motion is rapid, as the pressure on either side of the piston is the same and the spring exerts its full effort. After some motion, however, the air gets compressed, and before long the difference in pressure is enough to neutralise the force due to the spring, and the shutter leaves come to a full stop. At times a very curious effect is obtained; the momentum of the moving parts is such that the piston is carried past the point where the air pressure overcomes the force of the spring, and after coming to rest the motion

of the piston, and consequently of the leaves, is reversed; the case is similar to that of an elastic ball rebounding after collision with a fixed solid body. This rebound is most marked in the second diagram in the top row of figure 5.

This unreliability of the air brake will, of course, affect the speeds of any shutter in which such an arrangement is fitted, whether a trip gear is provided or not, but where the trip gear is omitted the efficiency is adversely affected as well as the reliability of the speed. It has, in fact, been observed in some shutters fitted with a trip gear that particular speeds are very uncertain—in one or two cases the exposure has at times been about five times as long as at others. It should, however, be understood that the remarks made above about air brakes are only intended to apply to a few particular shutters tested at the laboratory. Many of the shutters that have been examined show no appreciable variation.

It is evident from these results that there is a great deal of room for investigation into the suitability of brakes of various types for use in shutters. It would be of interest to know what results are obtained when oil is used in the dash pot instead of air. By a method of testing such as is used at the laboratory results in investigations of this kind may be obtained without difficulty, and it would certainly be of great interest to manufacturers if some one were enterprising enough to carry out such an investigation and publish the results.

We will now return for a few minutes to the focal plane shutter, and consider how it may be improved. Figure 9 shows how the efficiency varies with the equivalent exposure, when if a/f is the relative aperture, b the separation between plate and blind in inches, and v the velocity of the blind in meters per second, the values of ab/fv are 1/64, 1/32 and 1/16 for the three curves shown. Thus, for example, with a relative aperture of $f/4$, and a separation

of blind from plate of $\frac{1}{4}''$, the top curve will correspond to a blind velocity of 4 meters per second, and the middle one to a velocity of 2 meters per second; or if the separation is $\frac{1}{2}''$, the middle curve will correspond to a velocity of 4 meters per second and the lowest to a velocity of 2 meters per second. At comparatively slow speeds the efficiency is sufficiently good, but at the higher speeds under average conditions—$\frac{1}{2}''$ separation and 2 m.p.s. velocity—it is far from satisfactory. Figure 10 shows the efficiency for the higher speeds on a scale ten times as great.

The full curves in this figure correspond to those shown in the previous diagram. For all practical purposes these curves may be interpreted with the following table:—

Aperture *f*/4.	Aperture *f*/5·6.	Separation between blind and plate in inches.	Velocity of blind in meters per second.
Curve.	Curve.		
A	B	$\frac{1}{8}$	2
B	C	$\frac{3}{8}$	2
C	D	$\frac{1}{4}$	2
C	D	$\frac{1}{2}$	4
D	E	$\frac{3}{8}$	4
E	F	$\frac{1}{4}$	4

To improve the shutters at high speeds we have to increase the spring tension or reduce the separation between the blind and plate. The latter probably cannot be made less than $\frac{1}{4}''$, but by using metal at the back of the camera and for the dark slides, it should be possible to do this. There are, however, many reasons why a special effort should be made to increase the spring tension: among the advantages to be gained by doing this, besides increased possibilities in the way of speed and improved efficiency are lessened distortion, due to the decrease in the total time during which any part of the plate is exposed, and improvement

COMPARISON OF FOCAL PLANE AND BETWEEN LENS SHUTTERS USED WITH LENS WORKING AT APERTURE *f*/4

	Shutters giving highest speeds.	Focal Plane.	Between Lens.
At highest speed	Total duration of exposure	·0017	·0023
	Equivalent exposure	·0005	·0011
	Efficiency	·30	·47
	Ratio of equivalent exposures—longest / shortest ...	60	1000
	Most accurately marked shutters.		
At highest speed	Total duration of exposure	·0024	·005
	Equivalent exposure	·0006	·0027
	Efficiency	·25	·54
At lowest speed	Total duration of exposure	·072	·84
	Equivalent exposure	·071	·84
	Efficiency	99	1·00
	Percentage error of markings	14	15
	Ratio of equivalent exposures - longest / shortest ...	120	300
	Mean of results on all shutters.		
At highest speed	Total duration of exposure	·005	·0054
	Equivalent exposure	·0025	·0027
	Efficiency	·5	·5
At lowest speed	Total duration of exposure	·056	1·0
	Equivalent exposure	·056	1·0
	Efficiency	1·0	1·0
	Percentage error of markings	43	46
	Ratio of equivalent exposures—longest / shortest ...	33	500

of definition owing to the reduced effects of diffraction and any disturbance of the light by the edges of the blind opening. This difference in the definition between the two types of shutter working under the same conditions will only, of course, be apparent at the very highest speeds, if at all.

There is one defect possessed by the focal plane shutter as at present made for one particular class of work to which insufficient attention has been directed. In photographing birds and shy animals it is very desirable to have a noiseless shutter, or failing this a shutter in which the exposure will be over before any movement can have taken place due to the noise made by releasing the shutter.

At high speeds the time that elapses between the beginning of the exposure at the top of the plate and the conclusion at the bottom is independent of the length of time for which any one point of the plate has been exposed; it depends upon the length of the plate and the velocity of the blind. Thus with a half-plate, and blind velocity of two meters per second, parts of the plate will be exposed throughout an interval of 1/12th sec., though the "speed" may be only 1/1000th sec. Thus it is very likely—particularly with a reflex camera, in which a good deal of noise is made by releasing the mirror—that the subject will have started before the exposure is over, and a pose other than that desired, if not a blurred image, may result. On the other hand, with a between lens shutter, the exposure would be at an end within the time called the "speed" of the beginning of the noise. For work of such a character as this the between lens shutter is evidently very much to be preferred.

The table given above shows some of the results of these tests reduced to a form in which direct comparison is possible. These comparisons are given in three cases: (1) for the fastest shutters of each type, (2) for the most accurate-

ly marked shutters of each type, (3) the mean results of the examination of all the shutters. It is possible to attain with a focal plane shutter a shorter exposure than is reached by any of these between lens shutters, but the difference is not so great as might have been expected. In the average results there is in this respect nothing to choose between the two types.

The most remarkable difference shown by this comparison is in the range of exposure obtainable, as measured by the ratio of the longest equivalent exposure to the shortest equivalent exposure. The between lens shutter is in this respect out of comparison superior to the focal plane shutter.

In conclusion the author desires to thank the editors of some of the photographic papers, the manufacturers and others who have given permission for some particulars of tests on shutters carried out for them to be published; to express his indebtedness to Mr. A. F. Clayden, who has taken some of the records on which these results are based and has prepared copies of these diagrams for the Press; and to thank Dr. Glazebrook for the interest he has taken in the tests and for allowing these results and test diagrams to be placed before you.

In reply to questions raised in discussion, the author stated that he had not any figures to give showing the difference in the speed of the blind of a focal plane shutter at the beginning and ending of the exposure. With the between lens shutters examined the highest and lowest speeds were usually stated with fair accuracy, but the errors in the intermediate speeds were frequently considerably greater. No determination had been made of the speed of the blinds of the roller blind shutters (shutter near to the lens).*

* Since the date of this paper some determinations have been made of this: the values attained were very approximately 5 meters per second at the highest tension.—T.S.

ASTIGMATISM IN LENS SYSTEM.

By Mr. S. D. Chalmers, M.A.

Presidential Address delivered on 9th February, 1911.

The subject I have chosen for this address is the very old and perhaps hackneyed one of astigmatism, particularly in relation to systems of spherical surfaces. You are so familiar with the formation of astigmatic images by cylindri-

Fig. 1.

cal and sphero-cylindrical lenses that I need only refer to this for the purpose of settling the notation I will use. Fig. 1 shows the images of a point formed by a photographic lens of focus 160 and aperture 6 mm., having a +5 D cylin-

Fig. 2.

drical lens in front. The plate was focused to show the images formed at various places. You will note that the image may be a vertical or

horizontal line, or even a circle. These lines will be referred to as focal lines, and the circle as the circle of least confusion. On the whole this circle gives the best image of a point, while lines parallel to the focal lines are best reproduced at the corresponding focal line.

Fig. 2 shows the corresponding images of a small circular aperture when the value of the cylindrical lens is much smaller, .25 D. In this case one can hardly speak of focal lines.

Fig. 3 shows the images of a test chart formed by the systems (1). It is interesting as show-

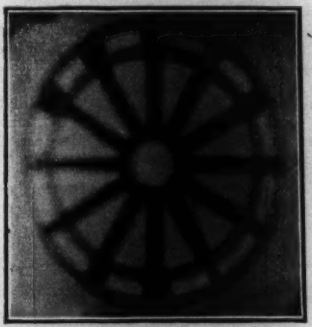

FIG. 3.

ing that there is no intermediate focusing position for which oblique lines are quite as sharp as the horizontal and vertical ones may be made.

Effects similar to the above may be produced by a lens or mirror when the object is not on the axis. Fig. 4 shows the two focal lines formed by a spherical mirror, the object being a small point 15° from the axis. In this case,

if the object O_1 be on the line CO_1 at right angles to the axis CB of the mirror, one focal line will fall on CO_2 produced, but the other and the circle of least confusion will lie off the plane of CO_2.

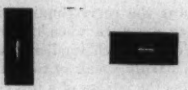

FIG. 4.

The diagram (fig. 5) shows these rays proceeding from a point O and focusing sharply at O_2. Thus O_2 is a focal line image of O. If the point O be moved back to O_1 the focal line will move in a corresponding distance.

In fact, for the object point O_1 we will have two focal lines, one on the plane CO_1 and the other on a circle of curvature 2 F where F is the power of the mirror.

FIG. 5.

The curvatures of the three lines at which the focal lines and the circle of least confusion are formed will be O or F−F, and 2 F or 3 F−F, and F or 2 F−F.

When the object is at infinity an additional complication comes in.

If three rays be drawn parallel to each other, as in fig. 6, they will not meet in a point, and at this place the actual shape of the image will be as shown in fig. 7. Figure 8 shows the images at three other positions of the plate.

FIG. 6.

It would hardly be right to describe these images as focal lines, but there is a sense in which this term may still be used. If the object be a vertical or horizontal line, the position of best definition will correspond with one of the lines, and this will be very nearly at the

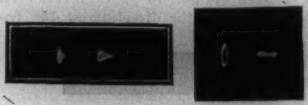

FIG. 7.

place of the focal line for a small aperture. The one-sided effect which is seen in these images is described as coma, and our results show that there is no coma in the case of an object placed near the centre of curvature of a spherical mirror.

Lenses.

Similar results may be obtained in the case of lenses, but the presence of the coma will depend on the shape of the lens. We will consider the images of points on a plane at right angles to the axis.

FIG. 8.

Fig. 9 shows the images of a point C_1 on the axis, at 5° and at 10° from the axis respectively, when the plate is not moved. The lens

FIG. 9.

is a plano-convex lens +8 D, with the convex side towards the distant object.

Fig. 10 gives best focus in each case.

Fig. 11 shows the images when the plate is moved towards the lens to give the best circle, the object being in the direction at 15° from

FIG. 10.

the axis. The next figures show similar effects for the same lens reversed.

So long as there is no stop away from the lens the two focal lines lie on curves whose curvature is $3F+F/n$ and $F+F/n$, where F is the power of the lens. If the lens be not thin the

expression F/n is replaced by the sum of the powers of the surfaces divided by n, and this expression is known as the Petzval curvature.

FIG. 11.

Fig. 12 shows four curves:

(*a*) The curve in which one focal line comes.

(*b*) The curve in which the best average definition comes.

(*c*) The curve in which the second focal line comes.

(*d*) Given by the Petzval curvature.

The curves are equally spaced.

FIG. 12.

The curvatures are:

$$3F + F/n.$$
$$2F + F/n$$
$$F + F/n.$$
$$F/n.$$

In the case of the mirror F/n is replaced by $-F$, and the curves were

$2F$
F
O
$-F$

FIG. 13.

When the surfaces are separated, or there is a stop placed in front of the lens, the curves (a), (b) and (c) are generally altered, so that the focal lines are moved towards or away from the lens; but these curves are still equi-spaced. It is easy to see what are the conditions under which the focal lines move.

FIG. 14.

In fig. 13 taking the intersection of the two outside rays which pass through the lens as the position of the first focal line, this will be :—

At Q_1 if the stop be at the lens (1).
At Q_2 if the stop be at (2).
At Q_3 if the stop be at (3).

You will notice that this stop effect is only important when the three rays do not pass through one point, *i.e.*, when there is coma in the image,

and that, by choosing the stop position, we can bring the focal line forward or move it back towards the focal plane.

By altering the shape of the lens we can alter the coma so that the central ray passes nearer the axis than Q_1 (fig. 14).

In this case a stop in front of the lens has the opposite effect, and moves the focal line further from the lens.

But we have seen that the four curves of fig. 12 are equally spaced, and that (*d*) does not alter, if only the shape of the lens is altered, the sum of the powers of the surfaces remaining the same.

Thus, if we wish to make the curves (*a*) and (*c*) coincide, we must choose the stop to move the points on the curve (*a*) back towards the curve (*d*). But with each shape lens there is a stop position which enables us to do this, unless there is no coma.

When the curves (*a*) and (*c*) coincide the focal lines are shortened till the image of a point is practically a point, and there is then no astigmatism, all lines being reproduced at the same place, though there may be a certain amount of coma. In practical cases the stop position being fixed by other considerations, the form of the lens must be properly chosen if it is desired to correct the astigmatism.

The expression which is obtained by dividing the power of each surface by the refractive index of the glass, and adding the results, is called the Petzval expression (P).

This gives the curvature of the curve (*d*), and it is only when the two focal lines coincide with each other and with (*d*) that this represents the best image position.

Still, the curve (*d*) is important, and it is always desirable to reduce the value of the expression (P), particularly in photographic lenses.

If we suppose for the moment that the lenses are all of the same glass, all thin, and all close together, we have $P = F/n$.

i.e., the radius of the curve (*d*) is $n \times$ the focal length.

If, however, the + and − lenses are separated, we can still retain the same total power, while increasing the actual power of the negative lens, and reducing the Petzval expression. In this way we can make the curve (*d*) approach the plane.

It is true that by using crown lenses of higher refractive index than the flints we can also slightly reduce the value of P; but in many practical lenses, the average value of the refractive index of the + lenses is nearly equal to the refractive index of the flint lens, and it is the effect noted above that is most important. The same effect may also be produced by separating the + and − surfaces of a thick lens, as in the Goerz lens.

Unless the astigmatism is corrected, it is preferable that the curve (*b*) should be plane rather than (*d*).

The astigmatism of the system will be corrected if the two curves (*a*) and (*c*) coincide with each other and with (*d*).

If the lenses are thin and in contact the difference of curvature is 2 F where F is the power of the system; but when they are separated this expression is replaced by twice the sum of the powers of the lenses, and the additional effects due to the stop not coinciding with the various lenses. We may proceed in two ways, either make the sum of the powers of the surfaces zero, and make all the coma effects balance out, or we may leave the positive lenses rather more powerful than the negative ones, and use the coma of the individual lenses to balance out the error.

In the Cooke lenses the first procedure was aimed at, each lens being made free from coma, and the powers of the plus lenses practically equal to the power of the negative lens; but in the final empirical correction of the lens some departures were made, and the plus lenses are rather more powerful than the minus lenses.

In the Unofocal, Steinheil aimed at the working out a single system of two lenses in which the + and − lenses were of equal powers and of the same glass. It was then necessary that the effects of the coma of the two lenses should balance out. Such a system can be constructed, but it has the defect that the half system shows coma, and it is difficult to get rid of this completely in the whole system.

Thus we can in various ways fulfil both the conditions necessary for the correction of the astigmatism and the reduction of the Petzval expression, if the sum of the powers of the plus surfaces is nearly equal and opposite to that of the negative surfaces.

It would therefore seem desirable to separate these surfaces as far as possible, getting as much power in the final system as we can, but there are other difficulties when the surfaces are

Fig. 15.

too widely separated. It is no longer fair to simply add together the errors of the two surfaces, because a ray which is not proceeding to the theoretical image of the first lens will meet the second lens in a different point, and so the errors of the lens will be modified. Thus, in fig. 15 the real ray passes along the dark line while the theoretical path is the dotted line. The real ray, therefore, meets the lens at a different point, and is differently deviated.

It is in consequence of these secondary defects that the design of photographic lenses of large field aperture is so difficult.

We find that when the astigmatism and curvature of field are corrected for a moderate

angle, say, 10° to 15°, serious errors occur for larger angles of field.

The curves of fig. 14 are not strictly circles, but generally bend back towards the focal plane; this is hardly appreciable in the curves for a single lens, unless very large angles are taken, but when the defects of one lens are almost compensated by those of another, the residual effects are easily seen, even for semi-

FIG. 16.

angular fields of 15° to 20°. This is evident from the curves in fig. 16, where the curves C_1 and C_2 represent the circles referred to above, and the other curves the real curves. The differences are shown to the right.

If we arrange so that the compensation is complete for objects 20° from the axis, there may be residual errors of appreciable amount at 10° and 30°.

It is necessary to either reduce the angular field for which the lens is to be used, or to select a type in which these secondary effects are small.

We can hardly talk of compensating these defects by others of the same nature, as this would require exceedingly complicated constructions with disadvantages far outweighing their merits. All we can do is to avoid the forms which introduce large errors, and so arrange the final correction of the system that the astigmatism curves give the best average result over the plate.

FIG. 17.

Small changes in the powers of the lenses or their separations will generally produce sufficient effect to alter the point of the field for which the astigmatic correction is best, and this enables us to choose the best average result.

Fig. 17 shows some typical astigmatism curves, as calculated by von Rohr.

You will notice that for the smaller angles of field the curves are rather in front of the focal plane; and this is characteristic of nearly all the photographic lenses. This means that the designer should slightly under-correct the error

of astigmatism and curvature of field for small angles, and the curves C_1 and C_2 of fig. 16 are generally slightly concave towards the lens.

In conclusion, I would like to express these astigmatism curves as a difference of dioptric power.

For the single thin lens of power F diopters used for an object 20° from the axis, the cylindrical difference is F $\tan^2$ D in diopters, or $\frac{1}{8}$ F approx.

In other systems this expression may be multiplied by a numerical factor depending on the design of the system.

It is interesting to note that the optical system of the eye, taking Tscherning's data for one case, gives a value of this constant of .19. Thus the error, assuming the surfaces spherical, is only about 1/5 of that of a lens of the same power used at the same inclination to its optical axis. The error, if we assume an angle D of 1° is only .16 D.

[This calculation assumes that astigmatism is not affected by the special structure of the lens.]

In the case of the single lens referred to above the constant is rather less than 1, but if the lens were of meniscus form with a stop behind the lens, the constant is steadily reduced as the last surface is made steeper, and may be made zero or even minus. If the stop distance be 20 mm. the constant is 0 for a value of the second lens surface of about -7 D.

From the nature of the astigmatism, it is clear that lenses need not give the same error when used for different object distances, and the correction of the astigmatism in spectacle lenses used for distance by choosing the most suitable form of lens will not generally mean the correction of astigmatism when used for reading; in fact, there is another form of lens more suitable for giving a wide field for reading purposes, in which the negative surface is much shallower.

As regards the curvature of field and astigmatism in photographic lenses, I have endeavoured to give the simpler theory and to show why and how far it does not represent the complete facts. A more complete theory might be given, but it is not yet possible to use the results in such a way as to make the computation of photographic lenses easy. At present we use the general ideas of this theory to enable us to express the results of trigonometrical calculations or measurements in such form that we can more readily determine the effect of any change in the lens. A more complete and simplified theory is much to be desired.

DR. W. ETTLES, F.R.C.S.Edin., EXHIBITS A NUMBER OF PIECES OF APPARATUS.

8th June, 1911.

A MAGNETIC *scotometer* for mapping blind areas in the field of vision—an important aid in the early diagnosis of glaucoma—consisted of a baze-covered disc mounted in a vertical plane. By means of a magnet moved over the back of the disc a small polished steel ball was caused to traverse the face or to remain fixed where desired, without any material attachment.

A double pinhole (for Scheiner's test), adjustable as to the distance between the holes, was formed by traversing a pair of slots inclined to one another over a second pair of slots parallel to one another. A modification of the same test suggested by Holth under the name of kinetoscopy was embodied in a machine for rotating a pinhole in a circle of adjustable diameter, and, of course, at such a rate as to secure persistence of vision. The application of this method to determining errors of refraction was fully explained. For rapid retinoscopy, as in the case of examining school children's eyesight, a machine had been invented and was shown by Dr. Ettles, which replaces the trial case, with a very great economy of time. It consists of a full series of trial lenses mounted on leaves that are attached by hinges to a flexible chain hung upon rollers. As each leaf previously hanging free, passes over the upper and smaller roller, it is erected into a vertical position, at right angles to the chain. Thus the successive lenses can be brought into position before the patient's eye very much more quickly than would be possible by any manipulation of an ordinary trial set. The principle of Mangin's mirror was applied to ophthalmoscopy, a plane retinoscope mirror being made virtually a concave mirror by simply clipping a lens from the trial case in front of it. Dr. Ettles' method of examining defects of colour vision by means of comparison spectra was demonstrated and explained.

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